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MISCELLANEOUS OBSERVATIONS

ON THE

EFFECTS OF OXYGEN

ON THE

ANIMAL AND VEGETABLE SYSTEMS;

AND

AN ATTEMPT

TO PROVE WHY SOME PLANTS ARE *EVERGREEN* AND
OTHERS *DECIDUOUS*.

BY

CLEMENT ARCHER, ESQ; M.R.I.A.

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Miscellaneous Observations

ON THE

EFFECTS OF OXYGEN

ON THE

ANIMAL AND VEGETABLE SYSTEMS;

ILLUSTRATED BY EXPERIMENTS,

AND INTERSPERSED WITH

CHEMICAL, PHYSIOLOGICAL, PATHOLOGICAL,
AND PRACTICAL REMARKS;

AND

AN ATTEMPT

TO PROVE WHY SOME PLANTS ARE *EVERGREEN* AND
OTHERS *DECIDUOUS*, IN THE CLIMATE OF
GREAT-BRITAIN AND IRELAND.

PART I.

BY

CLEMENT ARCHER, ESQ; M.R.I.A.

OF THE ROYAL COLLEGE OF SURGEONS, SURGEON TO THE LORD
LIEUTENANT'S HOUSHOLD, TO SWIFT'S HOSPITAL FOR
LUNATICKS, ONE OF THE SURGEONS ON THE
STAFF IN IRELAND, &c. &c.

> "It is by Experiment and rational Induction only that we can extort Truth
> "from Nature."

BATH, PRINTED BY R. CRUTTWELL;
AND SOLD BY C. DILLY, POULTRY, LONDON; GILBERT, DUBLIN;
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TO THE

RIGHT HONOURABLE

THE EARL OF ELY,

&c. &c. &c.

THE FOLLOWING ESSAY

IS INSCRIBED,

AS A SMALL TESTIMONY

OF THE RESPECTFUL SENTIMENTS,

MOST UNALTERABLE ESTEEM

AND GRATITUDE OF

THE AUTHOR.

BATH,
APRIL 1st, 1798.

c

In

Pa

ERRATA, &c.

In conſequence of a ſevere illneſs with which the Author was afflicted while this Eſſay was at preſs, the following Errata eſcaped notice.

Page 2, line 16, for ph*i*ſiologiſt read ph*y*ſiologiſt.
2, — 21, f. ph*i*ſiology, r. ph*y*ſiology.
5, — 24, f. element*s*, r. element, and put a comma after phlogiſton.
6, — 18, f. principle, r. principle*s*.
22, f. *in* rendering, r. *of* rendering.
7, — 5 from bottom, for *a*, r. *the*.
9, — 15, f. a*s*cends, r. a*c*cends.
11, — 15, dele the comma after colour.
15, — 13, f. *as*, r. *that*.
19, — 9 and 10, dele the parentheſis.
21, — 23, f. Becher, r. Be*e*cher.
25, — 22, r. and *apparent* death.
26, — 26, f. *glandules of*, r. *tubercles in*.
31, — 17, f. *but the*, r. *and the*.
47, — 17, f. c*i*aſſamentum, r. c*r*aſſamentum.
56, ——— in the note, f. The*t*is, r. The*s*is.
58, — 9, f. Dr. Hale, r. Dr. Hale*s*.
15, dele *it* at the end of the line.
62, — 13, dele *but* before taking.
63, — 16, ſemicolon inſtead of full-point at temperature.
64, — 1, f. become*s*, r. become.
67, — 13, inſtead of full-point at impurity, put a ſemicolon.
26, read, there was a generation of oxygen air, or a ſeparation of it.
77, — 10, f. experiment, r. experiment*s*.
80, — 26, f. give*s*, r. give.
86, — 20, r. and *in* which.
93, — 6, f. *was*, r. *were*.
94, — 21, f. Farenheit's, r. Fa*h*renheit's.
104, — 8, put a comma after churning.
104, — 13, f. hæmophthiſis, r. hœmoptyſis.

Page 51, add, as note to the word *azote*, the following:

* "A late experimenter on the air produced by leaves, aſſerts, that they do *not* yield azote during the night, but only ſimply ceaſe to ſecrete oxygen. (See account of Exp. on Vegetables, addreſſed to the Agricultural Society.) I have never made a ſingle trial on a plant growing in the dark, in which I failed to collect air much worſe than common air."

Preliminary Dissertation.

AS ſpeculations, however plauſible they may ſeem, or ingeniouſly ſpun out they may be, are, very properly, not much attended to, in this age of practical inveſtigation, until they have paſſed the *ordeal* of experiment—the only touchſtone to be relied upon, when we are about to enquire into the properties of *any* ſubſtance, but more eſpecially of ſuch ſpecies of *subtile* matter, as light, electricity, oxygen, hydrogen, &c. &c. — I, upwards of ten years ago, engaged in a chemical examination of the colouring matter of plants, and of the effects which the air, ſpontaneouſly emitted by the leaves of evergreens, deciduous trees, and herbaceous plants, collected whilſt growing in light, in ſhady places, by moonlight, and in the dark, has upon the blood of man and other warm-blooded animals; and having, in the proſecution of the inquiry, made certain obſervations, which I imagine may lead to a very curious diſcovery in natural hiſtory, viz. why the leaves of *some* trees and ſhrubs, which are of a peculiar conformation and conſiſtence, hereafter to be deſcribed, are *evergreen*, whilſt thoſe of *other* trees, ſhrubs, and inferior plants, which are of a different ſhape and texture, are deciduous, in this climate; I am induced to lay a faithful detail of the reſults of my experiments, and of the deductions I have drawn from them before the public; not that I am of opinion that they have fully eſtabliſhed my theory, but becauſe I think the publication of what I have done may be of uſe, though it ſhould only be to excite men of more genius than I can boaſt of, and of more leiſure than profeſſional avocations now allow me to beſtow on ſuch purſuits, to repeat and diverſify a ſeries of experiments which may throw new light upon ſubjects of a very intereſting nature to the Phiſiologiſt, the Electrician, the Chemiſt, the Botaniſt, and Naturaliſt at large.

Having mentioned that ſeveral of my experiments were made upon the blood with different kinds of air, I think it neceſſary, for the information of ſuch of my readers as may not be acquainted with phiſiology, to premiſe, that moſt of the kinds of air which are unfit for reſpiration, ſuch as fixed air, hydrogenous gas, azotic gas, &c. &c. poſſeſs the property of changing the blood of the arteries, whether it continues to circulate through the ſyſtem, or is let out of the body into any convenient veſſel, from its *florid red* colour, to a *dusky black* hue; while, on the contrary, *dephlogisticated* air, or *oxygenous gas*, (as it is now more generally called) is endued with an oppoſite power, and reſtores blackiſh blood to a beautiful vermilion colour, wherever it comes within its influence, as well through the air-cells, while it is circulating in the veſſels of the lungs, as after it has been let out of a vein, and expoſed to the external air: hence it is, that the blood, which is very dark coloured when it firſt paſſes from the heart into the lungs, is, immediately after it is ſubmitted to the action of the oxygen taken in with the common air in reſpiration, changed to *florid red;* and hence it happens alſo, that blood, which is black when it is let out of a vein, begins to aſſume a brighter colour on its ſurface, in a very ſhort time after it comes in contact with the oxygen contained in the atmoſphere.

Having premiſed theſe facts, I ſhall proceed to detail my experiments, and to comment upon the reſults of them.

The firſt experiment is rather foreign to the profeſſed object of the eſſay; but as it is very ſingular and intereſting, and as it gave riſe to the whole of the ſubſequent ſeries of experiments, I ſhall beg leave to lay it before my readers.

Having been called upon to bleed a lady of faſhion, of a very corpulent habit, to remove ſome indications of fullneſs ſhe began to be affected with, about the period of life at which ſhe ceaſed to menſtruate, and, not being able to find a vein at the uſual bleeding place in the arm, on account of exceſſive obeſity, I punctured one which preſented itſelf at the inſide of the wriſt, which bled pretty freely till it had more than half filled two ordinary-ſized tea-cups; after which it began to drop ſo ſlowly, that I had it in contemplation to put the lady's hand into warm water, as is practiſed when we bleed in the foot, to accelerate the flowing of the blood; but recollecting that there was an electrical apparatus at hand, and knowing that ſimple electrification quickens the motion of fluids through ſyphens, I determined to try whether it would produce a ſimilar effect on the lady's blood; I accordingly ſet my patient upon the inſulating ſtool, and having connected her to the prime conductor of a powerful machine, in favourable weather, the cylinder had not been made to perform many revolutions, before the blood began to flow faſter, though in a ſmaller ſtream, than it had done at firſt.—The following phenomena now attracted my attention: The lady (who was naturally very far from being affected by trifling inconveniences) inſtantly complained of a ſenſation of acute ſmarting at the orifice, from which the blood was flowing, and immediately after, I obſerved a beautiful fringe of exceedingly minute filaments begin to arrange itſelf around the lips of the little puncture, compoſed of the gluten of the blood, the more fluid parts having been ſeparated and driven off from it by the electric aura, which, conducted by the blood, flowed with it into a third cup I had placed to receive it.

Having obtained as much blood as was judged ſufficient, the lady was taken off the ſtool, and the hand was bound up, upon which the ſmarting ſenſation immediately ſubſided.

When I came to examine the blood, the next day, a moſt remarkable difference was perceivable between the appearance of the ſurface of the craſſamentum of the blood in the two firſt cups that flowed, and of that which had been obtained by the aſſiſtance of electricity;—that in the former exhibited the uſual *florid* appearance on the ſurface of the cake in each cup, whilſt the blood in the laſt cup continued *blackiſh* throughout the whole depth of the coagulum.

How are we to account for this remarkable fact? The chemical theory which prevailed at that time affords a clue to an eaſy ſolution of it; and though that theory is now become obſolete, I ſhall beg leave to inſert the explication it then ſuggeſted to me, in this place.

It was at that time the opinion of the celebrated Bergman, of Doctors Prieſtley and Franklin, and of many other eminent chemiſts and philoſophers, that there was a very great analogy between the chemical elements, phlogiſton and the electric fluid, inſomuch that ſeveral experiments were ſet on foot to aſcertain whether the ſame phenomena could not be produced by either of the ſubſtances; as for example, it being, at that time, the idea that the preſence of phlogiſton was the only thing neceſſary to give all the properties of metaliety, ſuch as ſplendor, maleability, &c. to the ores and calces of metals, it was tried whether the ſame effects could not be produced by means of the electric fluid (ſee Prieſtley on Electricity, Prieſtley's Exp. and Obſervations on Air, vol. iii. p. 279, and Bergman's uſefulneſs of Chemiſtry, p. 151) and it was at that time alſo, the opinion of Dr. Crawford, and ſeveral other eminent Phyſiologiſts, that the blood in the veins was of a darker colour than that in the arteries, in conſequence of its having depoſited the oxygen it had united with, in the lungs, and taken up a phlogiſtic principle in the courſe of its circulation through the ſyſtem.—Having ſtated theſe circumſtances, a ready ſolution of the phenomena muſt preſent itſelf to the moſt ignorant in matters of this nature. If electricity, and the principle of inflammability, have been proved to be but different modifications of the ſame principle, and the latter poſſeſſes the property of changing *red* blood to *black*, it will follow that the former muſt alſo produce a ſimilar effect.

Since I became acquainted with the property that electricity poſſeſſes in rendering blood, while flowing from a vein, ſo *indelibly black* that it cannot afterwards be changed to *bright red*, on being expoſed to the action of oxygen, I have very frequently tried the converſe of the experiment —that is, whether I could alter the colour of blood to which oxygen had imparted the *red* colour, by throwing a ſtream of the electric fluid from off pointed conductors upon its ſurface, and I have ſucceeded in ſeveral inſtances in favourable weather; but when the air is ſurcharged with humidity, and is conſequently in a ſtate to conduct off the electricity, and diffuſe it throughout the room, I have not been able to make the change to a blackiſh hue ſtrikingly perceptible, till after many repeated electrifications, on two or three ſucceſſive days, when incipient putrefaction may be ſuppoſed to have ſome ſhare in producing the phenomenon.

While I was engaged in proſecuting the foregoing and ſeveral other experiments with other matters, on veinous and arterial blood, I was frequently aſſiſted by a near relation, who had been afflicted with ſevere paroxyſms of convulſive aſthma from a very early period of his life, and it uniformly happened, that if he continued many minutes in the room while the electrical machine was at work, in damp hazy weather (when, as I have obſerved above, the air is invariably in a ſtate to carry off and diffuſe the electricity pretty equably through the ſurrounding air) he was certain to be attacked with a violent fit of dyſpnœa, or difficulty of breathing.

As fixed air is a compound of carbon and oxygen, I, at that time, entertained an idea, that electricity either poſſeſſed a power of precipitating fixed air from the atmoſphere, or (in conſequence of its poſſeſſing ſome property common to it and carbon) of generating it there by uniting with oxygen; and as we are informed by Sir J. Floyer, that inſpiring the vapour of fermenting liquors ſeldom fails to produce a fit of aſthma, in all ſuch as are habitually ſubject to that diſeaſe, I imagined it was to fixed air, and not to the electric fluid, we were to aſcribe the effect upon the reſpiration of my kinſman above alluded to; but having ſince, in a great variety of inſtances, ſhut up inſects, ſuch as flies, moths, butterflies, &c. in jars charged with electricity, and in mixtures of electricity and oxygen, in various proportions, without having been able to perceive the leaſt change in their reſpiration; and having alſo tried whether the tranſparency of lime-water (one of the ſureſt teſts of the preſence of fixed air) be capable of being diſturbed by mixtures of electricity and oxygen, without effect, I have long ſince abandoned the idea.*

I adopted the idea from Doctor Prieſtley, for whoſe authority, in experimental philoſophy, I *then had*, and in many inſtances, *ſtill have* the moſt profound reſpect; but the Doctor had, when he broached the idea, no leſs than *three* favourite hypotheſes to eſtabliſh:—1*ſt*. " That the electric matter either *is* or *contains* phlogiſton; 2*d*. That fixed air is a compound, conſiſting of phlogiſton and dephlogiſticated air; and 3*d*. That as the *electric light* is a modification of phlogiſton, *all light* muſt be a modification of phlogiſton alſo." (See Experiments and Obſervations on Air, vol. i. p. 186 and 280.) *One* favourite hypotheſis (I believe it is now pretty generally underſtood among philoſophers) is, for the moſt part, ſufficient to induce a fabricator of ſyſtems to *overſtretch* his facts to make them meet his theory.

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* I mean only with reſpect to being able to form fixed air with any mixture of the *electric matter* and *oxygen*. That I have proved to be impoſſible, but with carbon and oxygen I am of opinion an air may be produced as noxious to life as any with which we are acquainted.

The following argument, I humbly conceive, affords irrefragable proof that there are no two ſubſtances ſo diſſimilar in a *moſt eſſential* property as electricity and phlogiſton. When we bring a coal, a piece of wood, or any matter containing the principle of inflammability, into a ſtate of ignition, it emits both *light* and *heat*; but when the electric matter aſcends, as chemiſts term it, it gives out *light* only, without being able to impart the ſmalleſt degree of *ſenſible* heat either to the jar that is charged with it, or the ſubſtance that conducts it along its ſurface, or through its interior particles; but if we ſet a very inflammable ſubſtance (ſuch as ſpirit of wine, reſin, or gunpowder) within its influence, *heat* as well as *light* are immediately produced. Thus then, it appears, that the *only* thing that is wanting to give the electric fire all the characteriſtic properties of common culinary fire, is the *very* thing which Prieſtley aſſerts it either *is* or *contains*, viz. phlogiſton, or the principle of inflammability; for they *are* or *were* ſynonimous terms. I ſay *were*, becauſe phlogiſton, reſt its ſoul! is dead. It never poſſeſſed more than a ſoul without a body.

The ſame gentleman, my kinſman above alluded to, was alſo very liable to have his reſpiration violently affected, by going into a barn, while they were employed either in threſhing or winnowing corn of any kind, or by continuing in a chamber during the time they were making the bed. This is pretty generally the caſe with moſt aſthmatic people, and is occaſioned by the mechanical operation of the particles of duſt that are ſuſpended in the air; and hence it is that ſtone-cutters, ſawyers, cleaners of feathers, &c. are very apt to become aſthmatic.—(See Diemerbroeck, lib. ii. p. 443.)

Having, a little before I was engaged in making the above-mentioned experiments, become acquainted with the theory of Doctor Ingenhouſz,* concerning the property the leaves of plants *growing in light* poſſeſs of *purifying* the atmoſphere, and, on the contrary, of *injuring* it, while they vegetate in *ſhady places*, or in *the darkneſs of night*; I was induced to extend ſome of the Doctor's experiments to other objects than he applied them to, and ſubmit blood, *arterial* as well as *venous*, to the action of air diſengaged from the leaves of a great variety of trees, ſhrubs, and herbaceous plants, gathered while they were under the influence of intenſe ſunſhine; collected in places which were open to the acceſs of ſtrong light, but from which the rays of the ſun were excluded; pulled in the ſhade; by moonlight; and laſtly, procured from plants which had been, for a conſiderable time before the leaves were plucked, placed in utter darkneſs;—and having, for the purpoſe of making theſe laſt-mentioned experiments, ſhut up ſeveral pots of different kinds of green-houſe plants, (while they were in moſt luxuriant vegetation about Midſummer) ſuch as, Geraniums, Myrtles, Ledums, Dwarf Oranges, exotic Heaths, foreign Jeſſamines, Balm of Gileads, Roſe trees, Fuſchia Coccinea, &c. &c. in a dark but well-ventilated cloſet, I obſerved that in a few days, the plants began (ſome rapidly, and others more ſlowly) to be deprived each of them of the diſtinctive green colour, of its leaves, and of the peculiar odour which they emitted while they continued to grow in light; and towards the middle of the fourth week after they had been ſhut up, the leaves of all of them were changed from their natural green to a ſhade more or leſs approaching to white. The Geraniums, Balm of Gilead, Fuſchias, and the plants whoſe leaves are of a flimſy texture, and leſs gloſſy on their upper ſurfaces, were completely blanched, in the above ſpace of time; but the plants with more firm and ſhining leaves became only of a pale and ſickly-looking yellowiſh white, like graſs that has been covered by a haycock, a ſtone, a piece of timber, rubbiſh, or any other ſubſtance that excluded the acceſs of light for ſome time.

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* See Experiments on Vegetables, London 1779.

Having finiſhed the experiments for which the plants had been placed to grow in the dark; that is, having obtained air from the blanched leaves, and ſubmitted venous and arterial blood to its action, and having tried ſome of the ſame air, by the well-known teſt of nitrous air, (the reſults of which ſhall hereafter be adverted to) the plants were all replaced in their former ſituations in the green-houſe, where they gradually reſumed each the diſtinguiſhing green colour and peculiar fragrance of its leaves. The plants with firm and gloſſy leaves firſt, and thoſe with ſofter and leſs ſhining foliage in regular ſucceſſion, according (as it appeared to me) to the conſiſtence of the leaves; the firmer and more gloſſy recovering their green colour ſooner than the more ſoft and flimſy-leaved plants.

Before I attempt to account for the above curious and intereſting fact, I ſhall recur to the effects that air extricated from the leaves of plants that grow in light, darkneſs, &c. produces on the blood of man and quadrupeds, and enter into the particulars of the reſult of ſubmitting ſome of the ſame air to the nitrous teſt. But before I enter on this part of my ſubject, I think it right, as my book is intended for the peruſal of the unlearned among the fair ſex, as well as for the enlightened man of ſcience, to enter a little more minutely than I have hitherto done into an account of the ſeveral proceſſes the blood is ſubjected to in the lungs of animals; without the knowledge of which it will be impoſſible to underſtand the arguments which are to follow.

It has already been ſaid, that one of the properties of oxygen air is to change the *black* colour which the blood acquires in circulating through the *venous* ſyſtem, to a *bright red*, a proceſs which is carrying on in the lungs of man and of ſeveral other claſſes of animated beings, from the moment they *begin*, to that on which they *ceaſe* to breathe, in the following manner. The blood, which has been diſtributed to every part of the bodies of men, &c. *except the lungs*, through the arteries, by the unceaſing exertions of the *left* ventricle of the heart, becomes black in the courſe of its circulation through the machine, in conſequence, as has already been ſaid, of the phlogiſtic principle (carbon it is now thought to be) it enters into combination with, in its courſe; in which altered ſtate it is brought back by the veins to the heart, by the contractions of the *right* ſide (ventricle) of which organ it is forced into the lungs, where it lets go the ſubſtance which has made it *black*, and takes up another, which renders it *red* again, which ſubſtance was called *dephlogiſticated* air by Dr. Prieſtley, who firſt inveſtigated its properties; other philoſophers denominated it *vital* air, becauſe it is the only air that is capable of ſuſtaining the *vital* principle, or of ſupporting the combuſtion of inflammable matter; and, in conſequence of its having been diſcovered to be not only the *ſole* vivifying principle, but the parent of acidity, it has, by the modern inventors of a new chemical nomenclature, been named oxygen, from two greek words, οξυς *acidum*, and γινομαι, *gignor*, which ſignify to generate acids.

Here it is neceſſary to ſtate that the changing the blood from *black* to *red*, is not the only chemical proceſs that nature is every moment carrying on in that living laboratory the lungs; for it has been diſcovered by the late very ingenious Dr. Adair Crawford, that at the ſame time oxygen is effecting this change in its colour, it is alſo imparting heat to the blood, which is afterwards communicated to the reſt of the ſyſtem, according to the law which is known to regulate the diſtribution of that ſubſtance from bodies which poſſeſs more of it to ſuch ſurrounding matters as contain leſs, till they all become equally hot.

A competent knowledge of this beautiful theory is ſo intereſting, nay, indeed, ſo uſeful, particularly to mothers, to enable them to regulate the air in the chambers of their tender infants, and to make choice of proper places for their more grown children to exerciſe in, that I ſhall endeavour to make my readers acquainted with it, in as perſpicuous a manner as I poſſibly can.

The production of animal heat has, within this laſt century, been endeavoured to be accounted for, by three different theories; the mechanical, the pneumatical and the chemical. The mechanical phyſiologiſts, knowing that friction is capable of producing, or rather of evolving heat, were of opinion that animal heat was occaſioned and kept up by the attrition or friction of the red particles of blood againſt the ſides of the veſſels and each other in circulating through them.

The pneumatical philoſophers inform us, that the heat of animals is produced by the energy of the living principle, which they never attempt to give their readers a preciſe idea of; and this (ſays a late writer on the ſubject, Mr. Pearſon) beſtows pretty nearly the ſame kind of information on the ſubject, as certain among the ancients did, who aſcribed the operation of opium to its anodyne quality, without defining or explaining what that was, or on what it depended.

Within theſe few years, animal heat has been accounted for upon chemical principles, and this theory is therefore called the chemical. Dr. Crawford was the firſt who publiſhed this doctrine. It is highly probable, however, that he received hints, which might have given riſe to the theory, from the lectures of the late Dr. Irving, of Glaſgow.

To render this hypotheſis as clear as poſſible, we muſt advert a little to the theory of heat itſelf.—Heat, or caloric, as it is now called, is known to exiſt in all ſubſtances, in two different ſtates, as it were; that is, it may be quieſcent, fixed, and at reſt in bodies, in which ſtate it is not meaſureable by the thermometer, perceptible to our touch, or transferable from one body to another. In this ſtate it has been called *abſolute*, *latent*, or *elementary* heat. In the other ſtate it is at liberty, and is therefore obvious to our ſenſe of feeling, meaſurable by the thermometer, and transferable from one body to another. And further, to enable my readers to follow me, it muſt be made known to them, that when equal quantities of different ſubſtances have a given quantity of *ſenſible* heat thrown into them, their temperatures by the thermometer will appear different; for the ſame quantity of heat which raiſes one body a certain number of degrees, will raiſe another to a higher, or it may be to a lower number, and this is called the *capacity* of a body for receiving heat. Let us now proceed to Doctor Crawford's theory of animal heat.

This ingenious philoſopher conſidered heat to be a ſubſtance *ſui generis*, diſtinct from phlogiſton, and diſperſed throughout all the bodies of the univerſe, according to their various capacities for containing it. When this natural diſperſion of heat is diſturbed, ſo that one body is made to part with ſome of its natural quantity of it, and others are made to acquire it, then *ſenſible* heat is produced, and the body which has acquired a ſuperfluous quantity of *elementary* or *latent* heat, becomes *ſenſibly* hot; and this *ſenſible* heat is gradually communicated to the ſurrounding bodies, till they all arrive at the ſame temperature; whilſt thoſe bodies which have loſt a part of their *elementary* heat, become ſenſibly cold, and in order to reſtore the balance, attract part of the *free* heat from the neighbouring parts, till they are all in equilibrio.

The Doctor alſo ſuppoſes, and has ſupported the ſuppoſition by a multiplicity of moſt ingenious and very accurate experiments, that thoſe bodies which contain the greateſt abundance of the principle of inflammability, or phlogiſton as it was called, have the ſmalleſt quantity of *elementary heat*; hence it follows, that oxygen, which obtained its original name, *dephlogiſticated air*, from its having been thought to be wholly devoid of phlogiſton, *muſt* contain more *elementary* heat than any of the other elaſtic fluids of which atmoſpheric air is compoſed.

Having made you acquainted with theſe facts, let us proceed to the phenomena which Dr. Crawford imagines take place in the lungs of animals at each inſpiration, to produce and keep up animal heat.

The air is received into the lungs, containing a quantity of oxygen and (according to the foregoing hypotheſis) of *elementary* or *latent* heat, and meeting with the blood in the pulmonary artery highly ſaturated with phlogiſton, or carbon, (as it is now ſaid to be) which it had imbibed in the courſe of the circulation, the attraction of oxygen to the carbon is greater than that of the blood had been;

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this principle will therefore leave the blood to combine with the oxygenous air. By the addition of an inflammable principle, the air is obliged to depoſit a part of its heat; and as the capacity of the blood for containing heat is, at the ſame moment, increaſed by the ſeparation of the phlogiſtic ſubſtance, it will immediately attach itſelf to that portion of heat which had been ſeparated from the air; and by this double elective attraction, Dr. Crawford concludes the blood acquires *ſenſible* heat, which it communicates to the different parts of the body it comes in contact with, in the courſe of the circulation, according to the well-known law that temperature is imparted from bodies which poſſeſs more, to ſuch as contain leſs, till they arrive at preciſely the ſame degree of heat. Thus we ſee (ſays a late writer on this ſubject, Dr. Gardiner) that from the commencement of animal life, there is a conſtant generation and power of regulating heat; and from our living in a medium below the temperature of our bodies, there muſt be a conſtant conſumption of it. Till after the birth, it is natural to ſuppoſe (admitting the truth of Doctor Crawford's theory) that the fœtus muſt be indebted to the mother for its heat; but, when reſpiration commences, heat begins to be generated in the child by its own powers.— The proceſs of incubation will, I apprehend, ſufficiently illuſtrate this poſition.

In the rudiments of the chick, there are no powers capable of generating heat. It remains in the ſame ſtate as when the egg was excluded, till the proceſs of incubation commences, when it receives heat from the mother. This heat excites the principle of life into action; a more vigorous circulation takes place; the gradual extenſion and growth of the parts commence; the organs begin to unfold—to acquire ſtrength; and, in ſhort, (this myſterious proceſs) the formation and growth of the fœtus goes on by the aſſiſtance of the heat communicated by the mother.

During the time of incubation, the living principle every day increaſes in quantity and power, as the animal approaches to perfection, and its organs acquire the capacity of performing their functions; among which is the generation of heat, which laſt (according to this theory) cannot happen till the excluſion of the chick from the ſhell. After that, reſpiration commences, and the circulation and other operations are carried on with greater energy; and as the blood now paſſes through the great veſſels of the lungs, the animal has acquired the power of generating heat for itſelf. However, as this power has not yet arrived at its full vigour, the mother, by intuitive inſtinct, and as if conſcious of the tender ſtate of its offspring, gathers them under her wings from time to time, to impart to them a part of her natural heat. In the human race, reaſon and expeperience produce the ſame end. We employ the heat of fire, warm clothing, and lay the infant in bed with its mother; but as what is conceived to be the reſult of reaſon and experience, not infrequently induces the rich and luxurious to overſtep the bounds of nature and of ſound philoſophy, mothers, nurſes, and ſervants ſhould be particularly careful, when they place a new-born infant in a ſituation to aſſiſt its own feeble efforts to produce and ſuſtain its natural heat, not to exclude the free acceſs of freſh air, by too anxiouſly ſtopping up crevices in doors and windows, or by covering the head of the cradle with a blanket, or the like, or by drawing the curtains of a bed too cloſe; by which precaution (as it is evident from what has been ſaid, the infant muſt draw the principal and only natural part of its heat from the oxygen in the air it inhales into its lungs) a very fatal diſeaſe to infants—the eight-day fits, as it is called by nurſes—may be prevented; for it has been ſhewn by my ingenious friend Dr. Clarke, of the Lying-in hoſpital in Dublin, that the prevalence and baneful effects of that formidable complaint have been prevented by a proper attention to the ventilation of the wards of that very extenſive charitable inſtitution, over which he preſided with ſo much credit to himſelf and advantage to the community.—See the Doctor's paper in the Tranſactions of the Iriſh Academy.

Before I proceed to enter upon the detail of my experiments, I conceive it may be neceſſary ſtill further to premiſe a few more particulars reſpecting ſome of the theories which have of late *burst* upon the medical world like an impetuous torrent, from the ſeveral ſources of the new chemical ſyſtem; without a previous acquaintance with which, it would be impoſſible to make the objects I had in view in inſtituting the experiments, or the deductions I mean to draw from them, comprehenſible by any perſon who had not been bred to the medical profeſſion.

It has been ſaid above, that, according to the old chemical doctrine, the only thing neceſſary to ſmelt the ore of a metal, or ſemi-metal, or to revive their calces, was the addition of phlogiſton to the ore or calx; and the old chemiſts were of opinion, that this was effected, by ſubmitting the ore or calx to the action of fire in furnaces or crucibles; it being their idea that the inflammable principle of the combuſtible ſubſtance was abſorbed by the ore or calx; which they imagined was all that was wanting to ſmelt the former, or revive the latter.

Such was the chemical theory, as far as it related to metaliety, from the days of Becher and Stahl (the improvers of it) till within theſe few years, when ſome ingenious French chemiſts diſcovered that the phenomenon is to be accounted for in a very different way indeed; for inſtead of *adding* a ſubſtance *to* an ore or calx, to give it the metalline form, *they* have proved, that we muſt *deprive* the ore or calx *of* a property to produce the effect.

To elucidate this by a familiar example, I beg leave to ſtate, that when we are converting lead to a ſubſtance well known in the arts, red lead or minium, which is a calx of lead, the metal, during its calcination, acquires gradually a beautiful red colour, and when the proceſs is finiſhed, it will be found that the calx is conſiderably heavier than the metal from which it was obtained—a hundred pounds of lead being generally found to yield about a hundred and ten pounds of minium.—What a change has this proceſs produced! A hundred pounds of a bright ſhining metal, which was malleable, and the particles of which were ſo coherent that it was no eaſy matter to ſeparate them by repeated ſtrokes of the hammer, is converted into a hundred and ten pounds of a bright red powder, the particles of which have loſt the attraction of coheſion.

What is it which has effected this alteration?—The abſorption of oxygen from the atmoſphere during the proceſs of calcination, according to the new theory; the privation of phlogiſton, according to the Stahlian doctrine.

Thus then the new ſyſtem teaches us that it is the preſence of oxygen, and not the diſperſion of phlogiſton, which conſtitutes the difference between a calx and the metal which produced it; as it alſo does, that it is the diſſipation of oxygen, and not the addition of phlogiſton, which enables an ore or a calx to aſſume the metallic ſtate again.

An objection of no ſmall weight againſt the doctrine of phlogiſton was, that no perſon had ever been able to exhibit that wonder-working ſubſtance to our ſenſes, detached or ſeparated from the many various bodies which were ſaid to contain it; till the following experiment induced Dr. Prieſtley to imagine "that he had dragged the lurking traitor into day-light."

He took a certain quantity of minium, from which he carefully ſeparated all the air that was mixed with its particles, and ſetting it upon a piece of a broken crucible, which had been alſo well purged of its air, he introduced it, under water, into a glaſs cylindrical veſſel, filled with pure inflammable air, ſeparated in the common way with vitriolic acid and water from freſh filings of iron, and placing his apparatus in pretty ſtrong ſunſhine, he threw the focus of a tolerably powerful burning lens upon the minium; after a little time he was delighted to obſerve, that the water began to aſcend in the cylinder, a proof that the calx was abſorbing the air; and continuing the proceſs a few minutes longer, he obſerved ſeveral globules of lead begin to move about upon the fragment of the broken crucible. Here, ſays Prieſtley, we have an inſtance of phlogiſton in its *free* and *diſengaged* ſtate; it is nothing more or leſs than inflammable air, which the heat, produced by the concentered rays of the ſun, enabled the minium to abſorb, and thereby the metal was revived.

The new chemical doctrine affords a different ſolution of this fact. *It* maintains that inflammable air is no more than one of the component parts of water; and hence they call it hydrogen, which means the generator of water; the other part of water is oxygen.* The modern chemiſts, therefore, explain the phenomenon in the following manner: the heat excited by the burning glaſs diſengaged the oxygen in its aeriform ſtate from the minium, which, aſcending in the cylinder, attached itſelf to the inflammable air, and became water, while the minium, being, by this means, freed from what reduced it to the ſtate of a calx, or *oxyd*, (as all metallic calces are now called, from the effect that oxygen has in producing them) was at liberty to aſſume its metallic form.

Mr. Cavendiſh is inclined to think that the *imperfect* metals loſe their principle of inflammability, or phlogiſton, during calcination, and take in water in its ſtead; but with reſpect to the calces of *mercury* and the *perfect* metals, he thinks it ridiculous to decide whether the *mercury* and NOT the *water*, or the *water* and NOT the *mercury*, have loſt the principle of inflammability.

Oxygen, you have frequently heard, changes blood that has become black in the veins to a florid red colour in the lungs; and metals are converted into calces or oxyds, ſolely by the abſorption of oxygen.—Hence we can account for a very common effect of the calx of iron upon the complexions of patients afflicted with chloroſis, cachexy, leucophlegmacia, &c. which we have almoſt daily opportunities of obſerving, from being pallid and ſallow, ſoon become cherry-cheeked and florid, from a courſe of chalybeate medicines; which furniſh a portion of oxygen to the blood, in addition to what it ſupplies itſelf with from the air in reſpiration.

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* Water contains about 85 hundredth parts of oxygen, and 15 of hydrogen. See Fourcroy's Phil. of Chem. p. 34.

Another theory, which I find it neceſſary to lay before you, before I proceed to the experiments, is that of Dr. E. Goodwyn, concerning the connexion of life with reſpiration. This accurate chemiſt and ingenious phyſiologiſt is of opinion, that the cauſe of death, in ſtrangulation, drowning, and ſuffocation of every kind, is ſolely owing to the ſecluſion of *oxygen from the lungs, by the rope, the water, or particular kind of air by which the ſuffocation has been produced; and the Doctor propoſes, that the diſeaſe, or rather the cauſe of death, ſhould have a particular place aſſigned it in our books of noſology. He thinks it ſhould be thus defined:

Melanæma impedita ſanguinis venoſi, in arterioſum converſio, cujus ſigna, ſyncope et livor cutis.

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* Which I have already ſhewn is the only *vivifying* principle of the atmoſphere.

And he thinks it ſhould be divided into the following ſpecies.

Melanæma a ſubmerſione
——— a ſuſpenſione
——— ab inſpiratione aëris fixi
————— aëris phlogiſticati, &c.

See Goodwyn on the Connexion of Life with reſpiration.

Hitherto I have conſidered the effects which a *paucity*, or a *total want* of oxygen in the ſyſtem, has in bringing on diſeaſe, and cauſing death; it now only remains to explain certain inconveniencies, which are ſuppoſed to ariſe from an exceſs of it in the conſtitution, in one of the moſt deplorable and depopulating complaints, which the inhabitants of theſe iſlands are ſubject to—pulmonary conſumption of a particular ſpecies.—A theory, which though I am by no means ſo ſanguine as to imagine it will point out any *successful* modes of treatment in the more *advanced* and *later* ſtages of this kind of the diſeaſe, appears capable of enabling us to guard againſt its *insidious approaches*, and of *palliating* ſeveral of the moſt diſtreſſing ſymptoms, even in the more advanced and latter periods of the diſorder. To *cure* conſumption, after repeated inflammation and ſuppuration of the glandules of the lungs have taken place, after ſeveral attacks of hæmoptyſis or coughing up of blood, or when colliquative ſweats and diarrhœas have come on, is what no regular practitioner, I apprehend, can hope to atchieve by any medicine, ſolid, fluid, or aeriform.

This theory is the offspring of my friend *Dr. Thomas Beddoes*, a gentleman who has been indefatigable in his exertions to improve the healing art, by taking the new lights, which the modern diſcoveries in chemiſtry have thrown, not only upon the cauſe of diſeaſes, but on the compoſition of almoſt every article of the Materia Medica, as ſo many beacons to direct him towards more ſound and ſcientific modes of practice.

As I ſhall make frequent alluſions to this theory, in the directions I mean to give to valetudinarians of various deſcriptions, for a proper choice of ſituations to reſide and exerciſe in; I ſhall give a brief extract from the account the Doctor has given of it in his letter to Dr. Darwin, dated Briſtol Hotwells, 30th of June, 1793.

" The old medical writers divide conſumption into a great multitude of ſpecies or varieties. *Two varieties*, or *two species*, have lately appeared to me well marked; they have indeed been noticed by ſeveral of the beſt obſervers in our own and foreign countries; and it is not unlikely that they may require a very different method of treatment during their firſt ſtage.

" In the firſt, which may perhaps be termed the *florid conſumption*, (to which my theory is excluſively applicable) we perceive, eſpecially during its approach,

proach, towards its commencement, and at the time of the acceſſions of feveriſhneſs, a more vivid redneſs of the cheeks, as well as an extraordinary permanent redneſs of the lips, of the tongue and fauces. Indeed theſe parts, ſince I have been at pains to examine them very attentively, have uniformly ſuggeſted to me the idea of meat reddened by ſaltpetre. The eyes too, in ſuch caſes, are remarkable for their vivacity. The blood, diſcharged by epiſtaxis or hœmoptyſis, has a colour evidently more florid than uſual. In the latter indeed, which in healthy perſons is a rare occurrence, we have not many opportunities of remarking this difference in the colour of the blood; but in the ſpontaneous bleedings of the noſe that ſo frequently take place, both in incipient and confirmed phthiſis, we ſee the blood much brighter than when it flows from healthy perſons in conſequence of accidental violence."

From what has been ſaid, it muſt be very apparent, that the whole of the above-mentioned train of ſymptoms can ariſe from no other cauſe than too large a portion of oxygen in the blood, or (to ſpeak more medically) from an hyper-oxygenated ſtate of the ſyſtem, in this ſpecies of conſumption.

The mode of curing this complaint, or rather of preventing it from proceeding through the whole catalogue of melancholy ſymptoms which uſually attend it from its commencement to its fatal termination, is extremely ſimple, and depends entirely upon giving the patient ſuch articles of food in his diet, and ſuch kinds of air to breathe, as are known gradually to leſſen the ſuper-abundant quantity of oxygen in the ſyſtem.

A kind of *artificial* conſumption, which Dr. Beddoes informs us, he produced in his own perſon, in conſequence of having breathed a modification of air, containing too large a proportion of oxygen, affords a ſtriking illuſtration of the truth of his theory; as the mode of treatment, which the knowledge of the cauſe of his complaint ſuggeſted to him to adopt, is, of the efficacy of the modes of cure or prevention, which he has recommended to the public. I ſhall therefore beg leave to give the account in the Doctor's own words.

" After ſecuring a full ſupply of oxygen air, the firſt thing I undertook (ſays the Doctor) was to attempt to throw ſome light upon the nature of conſumption, by an experiment upon myſelf. Not having any thing of the phthiſical conformation, or the ſlighteſt hereditary claim to the diſeaſe, I thought I might venture very far in oxygenating myſelf without any great riſque; and it was impoſſible for me to obſerve the effects ſo minutely in another perſon. I accordingly reſpired air of a much higher than the ordinary ſtandard, and commonly ſuch as contained almoſt equal parts of oxygen and azotic air, for near ſeven weeks, with little interruption. I breathed it upon the whole ſometimes for twenty minutes, ſometimes for half an hour, and ſometimes for an hour in the day, but I never continued breathing it for above four or five minutes at any one time.

"I felt, at the time of inſpiration, that agreeable glow and lightneſs of the cheſt, which has been deſcribed by Dr. Prieſtley and others. In a very ſhort time I was ſenſible of a much greater flow of ſpirits than formerly, and was much more diſpoſed to muſcular exertion. By degrees, my complexion, from an uniform brown, became fairer and ſomewhat florid. I perceived a carnation tint at the ends of the fingers, and on all the covered parts of the body the ſkin acquired much more of a fleſh colour than it had before. I was rather fat; but during this proceſs I fell away rapidly, my waiſtcoats becoming very much too large for me; I was not ſenſible, however, of any muſcular emaciation, but rather the contrary. My appetite was good; and I ate one third or fourth more than before, without feeling my ſtomach loaded. In no long time I obſerved in myſelf a remarkable power of ſuſtaining cold. Except one or two evenings when I was feveriſh, I never once experienced the ſenſation of chillineſs, though cold eaſterly winds prevailed during great part of the time I was inſpiring oxygen air. I not only reduced my bed-clothes to a ſingle blanket and coverlid, but ſlept without inconvenience in a large bedchamber, looking to the N. E. with the window open all night, and with the door and windows of an adjacent ſitting-room alſo open. About the expiration of the above-mentioned time, I perceived ſome ſuſpicious ſymptoms. It was uncomfortable to me to ſit in a room at all cloſe. I frequently felt a ſenſe of heat and uneaſineſs in my cheſt; and my ſkin was often dry and hot, with burning in my palms and ſoles; my pulſe, which had hitherto ſeldom exceeded eighty, was above ninety in the evening. I took a journey of about 170 miles, the greater part in a mail-coach in the night, the reſt on horſeback. The roads were uncommonly duſty, and ſeveral circumſtances concurred to harraſs and fatigue me. On the way I met with a medical friend, who was much ſtruck with the fluſhed appearance of my countenance, and upon feeling my ſkin and pulſe, which varied from a hundred to an hundred and twenty, imagined that I was become hectic. I had now, though but ſeldom, a ſhort dry cough; but the ſenſe of irritation to cough required an almoſt conſtant effort to ſuppreſs it; this ſenſe of irritation was, as you may ſuppoſe, attended by dyſpnœa. I had alſo frequent bleedings at the noſe, an event almoſt unprecedented with me; the blood was of an unuſually bright colour; which was alſo ſeen in blood forced from the gums. Whenever I pierced the ſkin in ſhaving, the blood flowed in greater abundance than uſual, and was ſtaunched with difficulty.

"By keeping quiet and cool, and by the aſſiſtance, as I ſuppoſe, of a diet in which ſweet oil, butter and cream, bore a large proportion to the other articles, I very ſoon recovered my health. My inſenſibility to cold, as I had an opportunity of aſcertaining, during the evenings we have lately had (June 21ſt to 24th) ſtill continues; and the change of complexion will probably be permanent; it may at leaſt, I doubt not, be rendered ſo by inſpiring ſmall quantities of oxygen air from time to time."

Such are the outlines of this elegant theory. While I contemplate it, I know not which moſt to admire, the ſound chemical principles which give ſtability to its baſe, or the ſimplicity and harmony which add ſymmetry to the ſuperſtructure. It has been cavilled at, I hear, in private, in this country, and it has been more openly attacked in Germany; and yet I will take upon myſelf to affirm, that it ſtands foremoſt among the ſeveral theories, which the new ſyſtem of chemiſtry has given riſe to, in our times, in the ſcience of medicine.

I ſhall only ſay a few words on the compoſition of the atmoſphere, and on the manner of aſcertaining its purity by the teſt of nitrous air, before I proceed to the account of the experiments.

The air was for a long time ſuppoſed to be a ſimple, homogeneous elementary fluid; but by the diſcoveries of the moderns, it has been proved to be a compound ſubſtance: Dr. Prieſtley, and ſeveral other Engliſh experimental philoſophers, imagining it to conſiſt of *four* different elaſtic fluids, dephlogiſticated air, phlogiſticated air, fixed air, and inflammable air; while Lavoiſier and other French chemiſts maintain that it is compoſed of *two* aërial fluids only—oxygen air, and azotic gas—the former of which alone is reſpirable, and ſubſervient to combuſtion; the latter will not promote ignition, and, when breathed ſeparately, is ſo highly noxious, that it has obtained the name of azote,* which may be interpreted, deſtructive of life. The philoſophers of both nations, however, agree in this, that the part which *is* reſpirable and which aſſiſts combuſtion, amounts, pretty univerſally, to about a fourth part of the whole atmoſpheric maſs.

Perhaps the French chemiſts may have refined a little too deeply, when they expelled fixed air and inflammable air from the ſeats which they reſpectively filled in the atmoſpheric convention.

Fixed air, it muſt be allowed, is ſometimes preſent in the air even of very elevated ſituations, (to which, from its ſuperior weight, it is rather extraordinary it ſhould arrive.) Sauſſure proved it to exiſt in the air at the ſummit of Mont Blanc, by the well-known teſt of lime water; Sir William Hamilton, Magellan, and ſeveral other philoſophical travellers, met with it in a great variety of ſituations, in the vicinity of Naples, where it is probably produced by volcanic fire, from immenſe maſſes of calcareous matter within the bowels of that burning diſtrict; and it is natural to ſuppoſe, that wherever oxygen (which it has been ſhewn forms one part of fixed air) meets with carbon, or the etherial part of charcoal, (which is the other) they will immediately ruſh together, and conſtitute carbonic acid air, as the French now call fixed air. I could adduce a great many other arguments, to prove that fixed air is almoſt always to be met with in the atmoſphere of all ſituations, but I ſhall content myſelf with two. Dr. Bryan Higgins, in his very ingenious and uſeful work on calcareous cements, has proved, that the lime, ſand, water, and other ingredients which are uſed in the compoſition of mortar, concrete or harden moſt firmly, if they are mixed and uſed juſt at the time the quick or cauſtic lime is beginning to become mild by the abſorption of fixed air from the atmoſphere. My ſecond argument is, that fixed air precipitates quick or cauſtic lime from ſolution in water, wherever it comes in contact with it. Now as calcareous cements very ſoon concrete or harden, wherever they are uſed by the ſtone-maſon, bricklayer, plaiſterer, &c. &c. and as a pellicle or ſcum of mild lime may be found floating on the ſurface of the veſſel in which lime-water is kept in the ſhop of every apothecary in the univerſe, it muſt follow that fixed air is pretty generally preſent in the atmoſphere. And if inflammable air were not to be met with at leaſt in the upper ſtrata of our incumbent atmoſphere, the phenomena of lightening, ſhooting ſtars, fire-balls, and aurora borealis, could not be produced; for they are reſpectively occaſioned by the ignition of inflammable air, by the electric matter, in thoſe immenſe fields of vapour which are continually floating round the terraqueous globe.

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* This is Prieſtley's Phlogiſticated Air.

Diſtinct regions are aſſigned to each of theſe four kinds of meteor, with which if any of my readers ſhould wiſh to make themſelves acquainted, let them conſult a paper of Dr. Blagden, in the Philoſophical Tranſactions, for the year 1784, on a meteor which was ſeen in England on the 18th of Auguſt 1783. Let them alſo refer to ſome notes of Dr. Darwin, in his Botanic Garden, and to a paper of Mr. Kirwan, on Aurora Borealis, in the Tranſactions of the Royal Iriſh Academy, vol. ii.

The mode of aſcertaining the purity of atmoſpheric or any other air, by the teſt of nitrous air, ſtands foremoſt in the liſt of the many important diſcoveries which Dr. Prieſtley made in this department of ſcience. It is a very ſimple, and, when the teſt air is properly prepared, and the experiment accurately performed, an unerring proceſs. It is performed in an inſtrument called an eudiometer, of which ſeveral different kinds have been invented; one by Dr. Prieſtley himſelf, a ſecond by Magellan, a third by Abba Fontana. This laſt was that which I uſed in the following experiments: for a deſcription of it ſee Cavallo on Air. Of the inſtrument in general it will be ſufficient for my purpoſe to ſay, that it conſiſts of

three

three parts; 1ſt, a tube of between two and three feet long, and one-third of an inch wide, open at one end; 2d, of a meaſure for the airs; 3d, of a ſcale carefully graduated, according to the contents of the meaſure, into tenth and hundredth parts, ſo that one of the hundredth parts will be about one-ſixth of an inch. Thus provided, the operator is to go to work to prove the purity of the atmoſphere of the particular ſituation he wiſhes to aſcertain, in the following manner.

He is firſt to fill his tube with water, and invert it in a trough of the ſame fluid, and ſet it over a hole on the ſhelf of the trough; the conſequence of which will be, the tube will remain perfectly full of water, the preſſure of the atmoſphere upon the ſurface of the water in the trough being ſufficient to prevent the deſcent of the water in the tube. He is now to take the air meaſure, and fill it alſo with water; and if it be only the air of the place he is in he is about to try, he need do no more than ſimply pour out the water; for the air of the place, ruſhing in as faſt as the water flows out, will, when the meaſure appears to be empty, occupy the ſame ſpace as had before been filled by the water. The next thing to be done is carefully to convey the air contained in the meaſure under the ſurface of the water in the trough to the bottom of the tube. Matters being in this ſituation, by gently turning the hand, the air in the meaſure may be made to aſcend into the tube, within which it will cauſe a portion of the water to deſcend, exactly correſponding to the quantity of air that was contained in the meaſure. Now mark the ſpace which the air occupies, by applying the graduated ſcale to the outſide of the tube; the next thing to be done, is to take the ſame meaſure full of nitrous air, and introduce it alſo in the ſame way into the tube; gently agitate the tube, and, after waiting ſome little time, apply your ſcale again, and obſerve what diminution has taken place: for if you had introduced two meaſures of the ſame air, they would occupy, of courſe, twice as much ſpace within the tube as one would have done; but this will by no means be the caſe in the inſtance before us; for a meaſure of common air, and one of nitrous air, will not occupy as much ſpace as two meaſures of common air or two of nitrous would, but will fall conſiderably ſhort of it: and it is in proportion to this diminution that we judge of the purity of the air, the greater diminution denoting the greater purity of the air. Thus if one meaſure of nitrous air be added to one of any other kind of air, and the diminution be 1 $\frac{26}{100}$, we ſay that the meaſure of the teſt was 1.26. The *rationale* of this proceſs is as follows: Nitrous acid is compoſed of nitrous air and oxygen air; whenever theſe two airs therefore meet, they put off their *aëriform state*, and become *liquid* nitrous acid; which as it mixes with the water, no longer impedes the aſcent of that fluid in the tube.

I am aware that this minuteneſs of deſcription may appear tedious to many of my more learned readers, but I muſt again ſay, I write not ſolely for ſuch, but for the inſtruction and amuſement of a few female friends and acquaintances, who wiſh to become experimenters on the atmoſphere and the air elaborated by plants.

COMPARATIVE VIEW OF THE SPECIFIC GRAVITY OF DIFFERENT KINDS OF AIR.

	grains.
A cubic inch of atmoſpheric air, weighs	385
of dephlogiſticated air	420
phlogiſticated	377
fixed	570
inflammable	35
nitrous	399

See Lubbock's Theſis de Principio Sorbili, p. 127.

The preſſure of the air is moſt generally aſcertained by the barometer.

Sir George Shuckburgh found that the length of a column of air in a barometer was equiponderant to one $\frac{1}{10}$ of an inch of mercury, the ſpecific gravity of which was 13.6; the barometer ſtands between 29.5 and 30, and the temperature of our apartments is uſually from 50 to 60.

The ſpecific gravity of common air to water, is as		1	to 816
Dephlogiſticated air to common air		1103	1000
Inflammable	———	84-3	1000
Phlogiſticated	———	985	1000
Alkaline	———	600	1000
Nitrous	———	1195	1000
Fixed	———	1500	1000
Vitriolic acid	———	2265	1000
Hepatic air	———	1006	1000

See Kirwan on Phlogiſton.

The height of ſituations above the level of the ſea is alſo aſcertainable by the barometer; for as part of the preſſure of the column muſt be taken off every inch we aſcend, the mercury cannot ſtand as high in elevated ſituations as it does at the level of the ſea. Doctor Halley has proved that the mercury deſcends one-tenth of an inch for every 90 feet perpendicular height the inſtrument arrives at, in aſcending a mountain or other very elevated ground. But General Roy diſcovered, in his experiments on the great Snowdon chain of mountains in North-Wales, that conſiderable allowance muſt be made for the attraction of the mountain. The General on the 7th of Auguſt 1775, at nine in the morning, found the correct height of the barometer on Caernarvon Quay to be 30 inches 075, and on the peak of Snowdon 26 inches 418. At twelve o'clock, that on the quay fell to 30.43, and that on the peak to 26-405. —

The fall of the mercury on the plain was therefore only 1002dth part of the whole, and the fall on the mountain was 1232dth part of its original height: on the other hand, at two o'clock, the barometer on the quay roſe to 30.045, while that on the peak roſe to 26.415 inches correct height; therefore that on the Quay aſcended only 1.15021ſt part of the whole, whereas that on the Peak aſcended 1.2040th part of its height. See Kirwan on Climate.

EXP. I.

EXPERIMENT ON THE BLOOD OF A SHEEP WITH NITRIC ACID.

Having ſeen in a work of Mr. W. Higgins, on Phlogiſton, that he produced a ſubſtance very much reſembling bile, by taking equal quantities of blood and nitric acid, ſetting the veſſel containing the ingredients in a ſand-bath, and by adding ſmall quantities of water from time to time, as it was evaporated, till the whole of the acid ſhould be diſſipated along with the water; I repeated this experiment, and mixing two ounces of ſheep's blood freſh from the ſhambles, and the ſame quantity of ſtrong nitric acid, in a ſhallow flattiſh bowl of Wedgwood's ware, I placed the bowl in a ſand-bath, and having given directions to my aſſiſtant to add water every now and then, as adviſed by Mr. Higgins, till the whole of the acid ſhould be evaporated; and, after that, till the mixture ſhould arrive at the conſiſtence of the healthy bile of an ox. This was not effected without a great deal of labour, and at the expence of a conſiderable portion of time; for it was not till after the whole attention of my operator had been engroſſed for ten or twelve hours, that the proceſs was brought to a concluſion. I had now a liquor neither acid nor bitter in its taſte, and perfectly reſembling bile in its appearance. After a few days, I ſubmitted portions of this *artificial* bile to the experiments of Dr. Maclurg on the *natural* bile, and found that they did not differ in any material circumſtance in their properties.

N. B. We ſhall ſee in the ſequel of this Eſſay, whether this fact may not enable us to make ſome new phyſiological obſervations upon the ſecretion and properties of the Bile.

EXP. II.

EXPERIMENTS ON THE BLOOD WITH SALINE SUBSTANCES.

On the 28th of December 1786, having marked four tea-cups, A, B, C, D, I put into A Nat. vitriolat. dr. ij; blood, to the quantity of two ounces and a half, was let into it from the vein of a ſtrong young man labouring under a moſt ſevere catarrhous cough; in four hours there was not the ſmalleſt appearance of coagulation, the ſurface was not quite ſo florid as unmixed blood of the ſame perſon. Heat being applied, it did not begin to coagulate till the mercury roſe to 148, and then only in ſmall clots at the bottom of the cup. Coagulated into larger maſſes at 154, coagulum not very tenacious, rather friable, and very dark-coloured, fluid part of the blood of the ſame colour. Into B, I put dr. j of Nat. vitriolat. and let blood of the ſame patient into it, to two ounces and a half. In about an hour a buffy coat began to appear; at the end of four hours, this was very firm, but was not a complete ſtratum. The reſt of the blood was perfectly fluid, and where the buffy coat did not extend, was nearly of the colour of that in cup A. Began to coagulate into very ſmall clots, at 122, at the bottom of the veſſel. Moſt of it coagulated at 126. Thermometer roſe to 142, when the proceſs of coagulation ſeemed to be completely terminated, and there was not more of the whole coagulated than in A; colour of coagulum, and of fluid part, ſame as A. Into C was put dr. ij of Ol. Tart. per del. Blood to two ounces let in from the ſame perſon. In four hours blood perfectly fluid. Colour remarkably florid. At 110 pretty large coagulum formed, which was increaſed, though not very conſiderably, when the mercury roſe to 120. More of the blood remained fluid than in A or B; the fluid part was highly florid; the coagulated, viſcid and extremely black. Into D was put Ol. Tart. per del. dr. j. Two ounces of blood let in. It was perfectly fluid after ſtanding four hours. Colour more florid than that of A or B, but not ſo florid as C. At 108, began to coagulate faſt: at 110, moſt of the coagulum formed. Thermometer roſe to 120. Coagulum not ſo viſcid as that of C, but more ſo than of A or B. Coagulum very black, fluid part not ſo florid as that of C.

To part of the blood in cup A, water was added in various proportions. No coagula were precipitated. The ſame of B. Coagula, black and viſcid, were precipitated by water from C and D. Concentrated vitriolic acid, added to C and D, cauſed ſtrong effervеſcence, and the whole became very black, and continued fluid.

EXP. III.

EXPERIMENTS ON THE BLOOD OF A HEALTHY MAN, WITH DIFFERENT KINDS OF AIR.

Blood from a vein in the arm of a healthy young man, except that he had juſt received ſome ſevere contuſions in falling from a ſcaffold, was let into a glaſs jar, containing about eight ounce meaſures of very pure oxygen air, to the quantity of two ounces: ſame quantity of blood, from the ſame perſon, was received into a veſſel containing about eight ounces alſo of fixed air, eight ounces of inflammable air, and eight ounces of phlogiſticated air. About two ounces of blood from the ſame perſon were received in a tea-cup, and ſet by as a teſt of the experiment. After two hours the blood in the tea-cup had completely ſeparated into ſerum and craſſamentum, and had acquired the uſual florid red on the ſurface of the latter. The blood in the oxygen air had ſeparated into its parts alſo, and was florid at the bottom, as well as the top of the craſſamentum. The blood in the fixed air had ſhewn no diſpoſition to ſeparate, but on the contrary was pretty firmly coagulated: the coagulum was perfectly black throughout. The blood in the inflammable and phlogiſticated air was ſeparated into its parts, and the craſſamentum was very black throughout its whole ſubſtance, and never ſhewed the leaſt diſpoſition to be changed to a florid colour, by the action of the oxygen of the atmoſphere. The coagulum of the blood in the fixed air was taken out of the jar, and being placed upon the bottom of a wine and water glaſs, the glaſs was placed with its mouth downwards in a large cylinder filled with fixed air, ſo that the blood was expoſed to the free action of that aërial fluid. As the water aſcended in the cylinder, freſh fixed air was added from time to time. Matters continued to be managed in this manner for about a month, when the coagulum was taken out and examined. It appeared to be hard, black, and ſhining, as a piece of Kendal coal; was rather friable, was perfectly free from any putrid ſmell, and when a little of it was powdered and thrown upon live-coals, it was found to be perfectly uninflammable. The coagula of the blood in the phlogiſticated and inflammable airs, treated in the ſame way, were extremely offenſive in their ſmell, and were each of them inflammable, more eſpecially that which had been in the hydrogen air.

EXP. IV.

EXPERIMENTS ON THE BLOOD OF A SCROPHULOUS BOY, WITH THE SAME KINDS OF AIR.

A boy, in the Blue-coat hoſpital, Dublin, about thirteen years of age, fell off the banniſters, and was taken up in a ſtate of ſtupefaction and inſenſibility: I was ſent for to ſee him: I found him vomiting, and in a high ſtate of coma, with ſtertorious breathing; I immediately bled him very largely; and as I was endeavouring to aſcertain what external injury had been done to his head, I obſerved he was highly ſcrophulous. His ſkin was very fair, his hair flaxen, his eyes pale blue, his upper lip much ſwoln, and chapped almoſt through its centre. The ſublingual and ſubmaxillary glands were much enlarged and indurated. Upon examining the blood at the end of four hours, I found the texture of the craſſamentum rather flimſy, and the colour of its ſurface beautifully florid. Supplying myſelf with four pretty large pieces of it, I incloſed each of them in a little bag made of a very fine membrane prepared from the alantois of a cow, and moiſtening the outſide of each bag with a little ſerum of the ſame blood, I ſuſpended one bag in a jar filled with oxygen air, the ſecond in a jar of fixed air, the third in a jar of inflammable air, the fourth in a jar of phlogiſticated air. The next morning the blood in the oxygen air was perfectly fluid, and very red throughout its whole ſubſtance; the blood in the fixed air was very glutinous and very black; that in the inflammable and phlogiſticated airs was, neither of them, ſo fluid as that in the oxygen air, and both of them were very black throughout their whole ſubſtance. The blood in the fixed air, as in the former inſtance, did not become putrid at the end of a fortnight; while that in the hydrogen and azotic air were exceſſively offenſive on the fifth day; which was alſo the caſe with the blood in the oxygen air, but not in ſo great a degree.

EXP. V.

EXPERIMENTS ON THE BLOOD OF A HORSE, WITH AIR EXTRICATED FROM THE LEAVES OF PLANTS GROWING IN THE LIGHT, AND IN THE DARK.

Some leaves collected from a moſt luxuriant vine were put into a large glaſs cylinder, filled with and inverted in a large trough of tranſparent water; and the apparatus ſo diſpoſed was placed in ſtrong light, and pretty warm ſunſhine, in a window looking towards the Eaſt. In four hours a pretty conſiderable quantity of air had diſengaged itſelf from the leaves, and occupied the upper part of the cylinder. Into this I introduced a cube of the coagulum of the horſe's blood, which was rather black, as I had not ſuffered it to ſtand long enough to have its ſurface changed to red. In leſs than a minute, each of the four ſurfaces of the cube became of a bright florid red colour.

I extracted ſome air, in the ſame way, from the leaves of blanched celery, and introduced a ſimilar cube of black craſſamentum into it. At the end of ſix hours it continued very black. I took it out of the air, and expoſed it to the influence of the oxygen air, in the apartment, but it never became *reddiſh*.

EXP. VI.

EXPERIMENTS ON THE BLOOD OF A HEALTHY MAN WITH AIR EXTRICATED FROM THE LEAVES OF HEMLOCK, DROPWORT, OENANTHE CROCATA.

A few ounce meaſures of air were extracted from freſh leaves of œnanthe crocata, which is one of the moſt virulent of the vegetable poiſons of theſe iſlands. A lump of black craſſamentum was introduced into a part of it; it ſoon changed it to bright red; and when I came to examine a little of the remaining part of this air by the nitrous teſt, I found it to be as pure oxygen air as can be extracted from the leaves of any other plant.

EXP. VII.

EXPERIMENTS WITH AIR EXTRACTED FROM THE LEAVES OF DECIDUOUS TREES IN SUMMER AND AUTUMN, AND FROM EVERGREENS IN SPRING, SUMMER, AUTUMN, AND WINTER; WITH AN ATTEMPT TO PROVE WHY SOME PLANTS IN THIS CLIMATE ARE DECIDUOUS, AND OTHERS EVERGREEN.

I provided myſelf with a quantity of air procured from the leaves of the lime-tree, *tilia europea*, in the morning about Midſummer, in the year 1788, and tried it by the nitrous teſt, and found it to be very pure dephlogiſticated air. I obtained ſome air from the leaves of the ſame tree, collected at the ſame time of the day, and in as clear ſunſhine, about the latter end of October, in the ſame year, when the trees were beginning to aſſume their yellow tints; and ſubmitting it to the nitrous teſt, I found it was conſiderably more impure than the worſt atmoſpheric air. This proves that light is not the only thing neceſſary to give power to the leaves of deciduous trees, to elaborate dephlogiſticated air; they muſt alſo be ſucculent and full of ſap to admit of their performing that function.

I reſolved to try whether the leaves of evergreens were liable to have the ſame change produced in them at the approach of winter, and after a great number of experiments, I was agreeably ſurpriſed to find that they are not: for the leaves of all trees of the terrebinthinate kind, of laurels, Portugal-laurels, arbutuſes, hollies, ivy, &c. &c. poſſeſs the property of elaborating the ſame kind of air, in the day and night of winter, that they had done in the day and night of the preceding ſummer. I am therefore of opinion, that as the leaves of plants are green only during the time they are capable of emitting oxygen air, that plants with leaves of a firm ſtructure are evergreen, becauſe, in conſequence of that conformation, they are fitted to

reſiſt

reſiſt the greateſt cold of this climate, and to generate that elaſtic fluid throughout the year, in autumn and winter, as well as ſpring and ſummer.

While the old ſyſtem of chemiſtry prevailed, it afforded another explication of this phenomenon. That ſyſtem, I have ſhewn, maintained that there was great analogy between electricity and phlogiſton, and it alſo taught that it was to the phlogiſton connected with the rays of light, that plants are indebted for the green colouring matter, the eſſential oil, reſin, odour, &c. of their leaves. Electricity is more readily emitted and imbibed by pointed bodies, or bodies approaching towards a point, than by bodies of any other ſhape. If electricity and phlogiſton therefore had ever been ſatisfactorily proved to be modifications of the ſame principles, it would follow that bodies which are of a ſhape capable of abſorbing the greateſt quantity of the matter from which they derive their colour and odour, muſt have that colour and odour more deeply communicated to them than they are to plants of a different ſhape. The fact really is, that, in taking a ſurvey of the many various tribes of plants which compoſe the vegetable kingdom, we cannot help admitting, that the leaves of the moſt perfect evergreens are either very acutely pointed, approaching towards a point, or, which enables them to attract electricity with pretty nearly equal facility, preſent firm edges to the ſurrounding bodies. The leaves of all the fir tribe for inſtance, of furze *(Eulex Europæus)* broom, ruſhes, graſſes of every kind, and holly, are many of them *perfect needles* in their ſhape. While the leaves of all the myrtles, all the laurels, of phillaræa, ivy, arbutus, lauruſtinus, &c. have leaves of ſuch firm texture, that they (admitting that the phenomena could be accounted for according to the old chemical doctrine) were capable of doing with the edges of their leaves, what the others had done with the points of theirs. But as ſuch an explication of the fact cannot be admitted in the preſent improved ſtate of chemiſtry, we muſt endeavour to follow up the ſolution, according the new ſyſtem hinted at above, a little further to enable us to arrive at, perhaps, the truth of the phenomenon.

Is the elaboration of oxygen the only cauſe of the green colour and peculiar odour of plants in general? I believe it is. Why do you think ſo? Becauſe when plants are ſecluded from light (by the influence of which only they can perform that function) they become white in their colour, and vapid in their ſmell; and when they are reduced to this ſtate, inſtead of ſeparating pure oxygen from the air, they tranſmit azote. Do the leaves of deciduous trees and thoſe of evergreens yield oxygen air in nearly equal proportions daily, during the time the former are in verdure? The experiments I have made enable me to anſwer in the negative. Whilſt the leaves of leaf-falling trees are ſucculent, and as long as they are aſſiſted by the bright ſunſhine and warm temperature of ſummer and a part of autumn, they give out a greater quantity of reſpirable air by day, and more azotic-gas by night, than an equal weight of the leaves of any evergreen can be made to do; but as ſoon as ſlight autumnal froſts begin to ſet in, deciduous leaves loſe their vivid green colour and become yellowiſh; and when once the leaf of any tree aſſumes ſuch an appearance, or manifeſts any other ſymptom of approaching decay, it is a proof that it is no longer capable of ſeparating pure air. Why does not froſt produce the ſame effect on the leaves of evergreens that it does on thoſe of leaf-falling trees? Becauſe from the difference of the conſiſtence and conformation of the leaves of the two trees, it cannot affect evergreen in the ſame way it does deciduous leaves. In what manner does froſt deſtroy the leaves of plants? The obſervation of ſeveral years enables me to give the following account of this matter:—

A part of the dew or other humidity that falls upon vegetables towards night in autumn, is abſorbed by all thoſe that are furniſhed by nature with ſoft bibulous leaves, which moiſture, being converted into ice in the courſe of the night, deſtroys their organization and diſpoſes them to immediate decay, which, it has already been ſhewn, incapacitates them from tranſmitting oxygen air through their pores; but as moiſture, inſtead of being imbibed, is thrown off the ſmooth gloſſy ſurfaces of the leaves of evergreens, froſt can have no ſuch effect upon them, *they* therefore continue to give out oxygen from their air-veſſels, and are conſequently green throughout the year, as is the caſe with the leaves of trees, which are deciduous in this climate, in more ſouthern latitudes.

EXP. VIII.

EXPERIMENTS ON THE COLOURING MATTER OF THE LEAVES OF DECIDUOUS TREES AND EVERGREENS, WITH THE OXYGINATED MURIATE OF POT-ASH.

My late worthy friend, the truly ingenious practical chemiſt Mr. Robert Roe, having obtained a premium from the Linen-Board of the Kingdom of Ireland, for preparing a very ſtrong muriate of pot-aſh, by a very cheap proceſs, in the year 1791; I procured ſome of it from him in July in that year, and ſubmitted the leaves of the currant tree, the gooſeberry, the cherry, the pear, the apple, the aſh, the lime, the mallow and muſtard, to its influence in the morning about eight o'clock; and at the ſame time I put leaves of laurel, lauriſtinus, arbutus, ivy, and holly, into an equal quantity of the ſame liquor. It was remarkably ſtrong; for by an improvement on the indigo teſt of Mr. Decroiſille, it was found to be 700 ſtrong. On the next morning I obſerved that the leaves of the currants, gooſeberries, cherries, and of all the other deciduous trees, were bleached to the moſt delicate white I think I ever beheld; and though the liquor was ſo ſtrong, it did not appear to have deſtroyed the ſtructure of the leaves; while the leaves of the laurel, lauriſtinus, arbutus, and other evergreens, were only changed to a pale yellowiſh white. And though I left them in the bleaching liquor for three whole days, they were never completely bleached. This is a proof that the leaves of evergreens not only elaborate oxygen more ſlowly, but part with their colouring matter with greater difficulty and reluctance than the leaves of deciduous trees do.

EXP. IX.

EXPERIMENTS ON AIR DISENGAGED FROM ROOTS, VIZ. POTATOES, TURNIPS, AND PARSNIPS.

It having been proved by Dr. Ingenhouſz, that roots, when left out of the ground, yield foul air by day as well as night, and infect the ſurrounding air, (ſee Ingen. p. 53) I ſupplied myſelf with ſome air from the roots of potatoes, turnips, and parſnips, and found that *that* which was yielded by the *firſt* and the *laſt*, contained no oxygen; but the air which iſſued from one or two of the turnips was pretty nearly as much oxygenated as the air of the room in which the experiment was made. How are we to account for this fact? The part of the rind of the turnip, which is above the ſurface of the earth, is uſually of a greeniſh or purpliſh colour, and when it is either one or the other, that part of the rind is capable of elaborating oxygen; while the part which is covered up by the earth, and conſequently ſecluded from the influence of light, yields very foul air.

EXP. X.

EXPERIMENT TO PROVE THAT THE IRRITABILITY OF PLANTS IS OWING TO THE OXYGEN THEY CONTAIN, OR ARE ELABORATING.

Having, in the proſecution of my experiments, diſcovered that the leaves of the mimoſa pudica, and and the antheræ of the common barberry (*berberis vulgaris*) are ſenſitive only while their reſpective plants are elaborating oxygen air; I determined to ſeclude a plant of the mimoſa, and a peduncle of the berberis, from the influence of light, till they ſhould be deprived of their colouring matter, and conſequently of their power of producing oxygen. At the end of fifteen or ſixteen days the mimoſa was blanched nearly white, and was totally deprived of its irritability. About the ſame time the bunch of barberry bloſſoms, though it was not much changed in its colour, was totally deprived of the ſenſibility of its antheræ; for they might be approached by the ſharpeſt pair of ſciſſars, without appearing to ſhrink the leaſt in the world from the points of that inſtrument.

The peduncle was not ſeparated from the parent tree, but was ſecluded from light, by incloſing it in a pickling crock of ſtrong black earthen ware, ſuſpended among the branches, and ſo diſpoſed as effectually to ſhut out every ray of light from the incloſed bunch of bloſſoms.*

EXP. XI.

EXPERIMENTS ON THE AIR PRODUCED BY THE FLOWERS AND PETALS OF PLANTS.

It has been found by Dr. Ingenhouſz, and many other experimenters, that the air produced by flowers is much worſe than the atmoſphere. Dr. Heiſter gives us an account of a Nobleman, in Germany, who was inſtantly ſtruck dead upon going into a room, the chimney of which had been cloſely ſtopped and moſt gorgeouſly garniſhed with a great profuſion of the white lily (*lilium album.*) The deadly effects of that moſt extraordinary tree in the iſland of Java,

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* Dr. John Peſchier, in a theſis publiſhed at Edinburgh, in June, 1797, has favoured the world with ſome very intereſting experiments upon the effects which oxygen poſſeſſes in communicating irritability to plants.

(if there be ſuch a tree, which Sir G. Staunton doubts, after the moſt diligent enquiry on his route to China) the Bohen Upas, may poſſibly be occaſioned by its being placed by the hand of nature in ſuch a ſhaded ſituation as to admit of its giving out azote only, for it has been repeatedly proved that the moſt poiſonous plants which grow in this climate yield vital air while they grow in the light.

About the middle of ſummer 1788, I began to make experiments on air produced by the corollæ, petals, antheræ, and ſtigmata of a great variety of field and cultivated flowers, and found uniformly, that they all produce air of a very noxious quality indeed. Theſe facts furniſh us with a variety of uſeful practical information. They enable the judicious phyſician to determine in what claſſes of diſeaſe he may leave a quantity of flowers in the chambers of his patients without danger, it may be with advantage, for it is now proved beyond a doubt that breathing air leſs pure than the atmoſphere is of uſe in ſome diſeaſes of the lungs.

ENQUIRY INTO SOME PARTICULARS RESPECTING THE CIRCULATION AND DISTRIBUTION OF THE SAP IN PLANTS, WITH AN ACCOUNT OF THE MANNER IN WHICH THE LEAVES OF PLANTS PURIFY THE AIR.

It has for a long time been a deſideratum with the curious in vegetable ſtaticks, to aſcertain whether a regular flow and aſcent of ſap, from the roots of a tree into its trunk and branches, is conſtantly going forward, from the time the ſap firſt begins *to riſe* (as it is called) in the ſpring, to that at which it *deſcends* (as it has been thought to do) towards the commencement of winter. The experiments of the celebrated Dr. Hale prove, that ſuch a circulation does actually exiſt in many inſtances: indeed it muſt be always going on, as long as every part of the trunk and branches continue in a temperature ſufficiently high to keep the ſap in a proper ſtate for circulation; but when one part of a tree is in *such* a medium, and another in one conſiderably lower, it the ſap will flow through the part which is in the higher temperature only, while it will be at reſt, or in a ſtate of ſtagnation, in thoſe parts which are in lower degrees of heat.

The following experiment of Dr. Ingenhouſz affords ſufficient evidence of the truth of this poſition. The Doctor, I do not know in what year, (for I quote the experiment from the recollection I have of an account given of it in the Edinburgh Medical Commentaries) took two apple-trees, of the ſame age and pretty nearly of the ſame ſize, which had been planted in tubs, for the purpoſe of forcing the fruit, in the Imperial hot-houſe at Vienna; and having placed one tub at the *inside* of the houſe, and the other at the *outside* of it, pretty cloſe to the glaſs, he conveyed a large branch through the glaſs (ſecuring the hole with luting) from the tree, the root of which was placed *within* the hot-houſe, into the external air, in ſevere weather in January. And (as the converſe of this experiment) he brought a ſimilar bough from the tree, the root of which was in the external air, *into* the hot-houſe, through a hole in the glaſs which was ſecured alſo with luting. After a few days, the boughs of both the trees which were in the warm medium began to germinate, afterwards to put forth bloſſoms, and laſtly to form fruit; while the boughs of both trees, which were expoſed to the rigour of the ſeaſon, remained in the ſtate that trees of the ſame kind which were growing in the open air continued to be in, without ſhewing the ſmalleſt ſigns of vegetation. From this experiment, it appears that the ſap, which I think does not deſcend towards the root at the approach of cold weather, but is frozen, as it were, concreted, or reduced to ſuch a ſtate, by the operation of the cold, as to render it unfit for circulation; may be thawed or liquified, by a proper temperature, ſo as to render it capable of circulating in the veſſels of that part of the plant, and of *that only*, which happens to be placed in the temperature high enough to keep it in a ſtate of liquefaction. If the ſap, when once it is liquefied, in or near the root, does in every inſtance find its way to the extremeſt branches of a tree; why did it not flow from the root of the apple-tree which was placed *within* the hot-houſe into the branch that had been carried through the glaſs, into the *open air*, as well as it had done into the branch which was *within* the houſe? and if a circulation of ſap from the root of a tree into its branches be abſolutely neceſſary to produce vegetation in thoſe branches, how did it happen, that ſuch a circulation could not be traced from the root of the tree, which had been placed at the *outside* of the hot-houſe, into the branch which had been conducted through the glaſs *into* it.

The following experiment, which I made at my brother's at Fairview, in the county of Dublin, in ſpring 1795, will prove, that the ſap does *not* always begin to flow from the roots of trees towards their branches, but that it very often circulates in the branches of trees, while it remains perfectly at reſt nearer to the earth.

EXPERIMENT ON THE SAP OF A YOUNG BIRCH-TREE.

Towards the end of March, I made deep *tranſverſe* inciſions into the bark of a tall young birch tree, at different heights, and in different aſpects. The firſt inciſions were made within a few inches of the earth, one to the S. one to the N. one to the E. and one to the W. This part of the tree was very much ſhaded by a thick cluſter of lauruſtinus, which grew at the foot of the birch. The ſecond inciſions were made in the ſame aſpects, about four feet ſtill higher up, above the ſhade of the lauruſtinus. And a third ſet in the ſame directions, about three feet higher up. The ſeaſon was uncommonly ſevere; there had been falls of ſnow on the 17th, 18th, 19th, and 20th of March, and many parts of the hills in this mountainous diſtrict were covered with ſnow till near the middle of May. Vegetation was therefore very backward that ſpring; and it was not till the 8th of April, when the wind changed from the N. W. to the S. E. that I obſerved any of the inciſions in the bark of the birch begin *to bleed*. The inciſion from which the ſap began firſt to diſtil in fine pellucid drops, was that which was made towards the S. in the higheſt ſeries of inciſions. The 13th of April was fine mild weather, with much ſoft, thick, miſty rain; wind S. E. The buds of the elm, horſe-cheſnut, ſycamore, white-thorn, lime, &c. &c. &c. appeared *to burst forth*, as it were, into a ſtate of vegetation, while we were contemplating them. On the 14th of April, I obſerved that all the inciſions in my middle ſet began to bleed in a copious ſtream, while the lower inciſions continued perfectly dry. It occurred to me to try whether I could, by artificial means, prevent the ſap from flowing from the middlemoſt inciſions; and as I ſuppoſed this could be effected by cold, I got ſome unmelted ſnow, and mixing it very well with wet ſtraw, I bound it round the trunk of the tree, a little above and a little below the inciſions, but taking care not to let any of the ſnow get into them; the conſequence of which was, that, though the weather continued to be moiſt and warm, the inciſions ceaſed to bleed before the next morning, the cold of my bandage having condenſed the ſap. The upper ſet of inciſions continued to bleed very copiouſly, becauſe they were in a temperature ſufficiently warm to keep the ſap in a proper ſtate of liquefaction, to admit of its circulating through its veſſels. The lower inciſions were ſtill dry. I now removed the lauruſtinus, which ſhaded *these* inciſions from the ſun, and refreſhing them by paring away a little of their edges, (which I had often done before, leſt they ſhould become too dry to give exit to the ſap) in eighteen hours after, *this* ſet of inciſions began to bleed alſo, but not in a more copious ſtream than the others had done; which it is natural to imagine muſt have been the caſe, if the whole ſap of the tree aſcended from the root into its trunk and branches. I removed the ſnow and wet ſtraw from the middlemoſt inciſions, and they began to bleed again in leſs than twenty-four hours.

I have often put a ſtop to the bleeding of an inciſion in a birch-tree, by cauſing æther to evaporate from around the edges of it, which generated cold enough to arreſt the flowing of the ſap through the veſſels which led to the inciſion.

Thus, we ſee, the ſap is always preſent in every part of a tree, either in its concrete or fluid form. It may be made to flow, by capillary attraction, in any part of a tree which happens to be in a proper temperature. While it is perfectly at reſt, or in a ſtate of congelation, in other parts of the ſame tree which may be in a colder medium.

Trees and plants of every kind are capable of abſorbing their peculiar pabulum, or nouriſhment, from the air, by their leaves, as well as from the earth by their roots; and hence it happened, that a ſprig of mint, which had no root, grew luxuriantly in putrid air, with Dr. Prieſtley; and hence it happens alſo, that thiſtles (*carduus lanceolatus, et criſpus*) Senecio Jacobæa, &c. &c. &c. when they grow to the leeward of, and at no great diſtance from large dunghills, in ſpring when the wind blows from the ſame quarter for ſeveral weeks together, becomes very rank and luxuriant, though the fibres of their roots are far without the reach and fertilizing effects of the manure. It is enough, if *its vapour* be brought within the attractive power of the abſorbent veſſels of the leaves: it is imbibed, aſſimilated, and in a very ſhort time in a ſtate of circulation, giving vigour and growth to the tree or plant, and enabling it to perform a moſt important function of the vegetable creation to man—the purification of the atmoſphere, by continually pouring oxygen into it in the following manner:* The whole of the air that comes in contact with the trunks and branches of the trees, is abſorbed by the air-veſſels, which are every where thickly beſpread upon their bark and leaves. The *impure* part, or azote, is nutritious to the plant, and is therefore retained within it for its ſupport; while the vital part, or oxygen, not affording any nouriſhment to vegetables, is thrown back into the common ſtock of air, freed from a conſiderable quantity of impurity with which it had been mixed at the time it was abſorbed by the air veſſels. But this proceſs can be conducted by nature only during the day-time; for the leaves of trees, &c. it has been demonſtrated, cannot elaborate oxygen without the aſſiſtance of light, nor can the whole of the azote be converted to the nouriſhment of the plant, by its innate powers, during the continuance of darkneſs, but is then given back into the air.

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* This is the theory of Sir Benjamin Thompſon.

What a reverſe does the approach of night produce? Oxygen air cannot now be generated, nor can azotic be prevented from mixing again with the element we breathe. We might be induced, therefore, to think that plants, which are ſo beneficial to man in the day, from the quantity of pure air they prepare for him, rather more than counter-balance their good effects, by the portion of impure they pour forth in the night. But this is not the caſe; for it has been clearly demonſtrated, that there are diſeaſes, and ſymptoms of diſeaſes, which ariſe from too great oxygenation of the ſyſtem, which are moſt eaſily removed by breathing air of an oppoſite quality: as for example—If a man exerciſes too much in the day, his reſpiration is quickened, he neceſſarily inhales more air, and conſequently more oxygen, within a given ſpace of time, than he does when he uſes ordinary exertions only; he therefore feels reſtleſs, heated, fluſhed:—but this commotion ſometimes ſubſides in one night, during which, we know, he breathes much worſe air, the hyper-oxygenation is counterpoiſed, and he is as well as ever the next morning.

I ſhall beg leave to add another caſe, which will put the goodneſs of Providence, in cauſing various kinds of air to be produced in different ſituations, of one and the ſame field perhaps, in a more ſtriking point of view than it has hitherto been.

About

About two o'clock, in ſummer weather, I was walking with a lady and two beautiful children, girls eight or nine years old, with light hair, blue eyes, fair ſkins, very florid cheeks, and remarkably white teeth; in ſhort, perfectly healthy, as might be ſuppoſed by ordinary obſervers; but in fact, children of ſuch a deſcription have too frequently a prediſpoſition to become ſcrophulous and conſumptive. Our walk led to a large piece of water, the banks of which were planted with a double row of large oziers, whoſe leaves are known to yield oxygen air in greater quantities than thoſe of almoſt any other tree or ſhrub. The children, though they did not exert themſelves more than they had done in the former part of their walk, which was through a plantation of hollies, laurels, and ſilver-fir-trees, began to breathe very quick, but without apparent difficulty, and to have a much more florid red imparted to their cheeks, and to complain of great internal heat. "The children have, I fear, overwalked themſelves," ſaid the lady:—'No, madam, they have, it is more probable, only remained too long in *this* walk; let us go back into another part of the plantations.' We did ſo, into a walk planted with large arbutuſes, bays, laurels, and other *evergreens* only; and though they continued to walk at the ſame rate, and with the ſame ſpirit as formerly, the children in a ſhort time began to breathe ſlower, and to complain leſs of heat, but the very high colour of their cheeks continued throughout the remainder of the day. "The ozier walk was too ſultry," ſaid the lady. 'It produced too much oxygen air, for people already too highly charged with it,' the philoſophical chemiſt perhaps would ſay. As I mean to advert to this ſubject in another ſection of this eſſay, I ſhall ſay no more of it here.

While leaves are employed in the beneficial proceſs of keeping up the purity of the air, is it ſuppoſed that they do no more than merely ſeparate oxygen air from the atmoſphere, and give it to us again unmixed and uncontaminated with any impurity. Or are they capable of *generating* pure air? The following experiment of Dr. Prieſtley clearly proves that though the former is moſt general, the latter *may* ſometimes be the caſe. The Doctor incloſed mice in a jar of air, till they died for want of a proper ſupply of oxygen; he then introduced a ſprig of mint into the jar, to ſee what effect air in which animals had died would have upon vegetable life. The mint grew moſt luxuriantly, and the water ſunk in the jar; a proof that inſtead of a loſs, there had been an acceſſion of air, in conſequence of the growing of the plant within the jar. To aſcertain the nature of this air, he introduced *other* mice, and they lived nearly as long as the former ones. Here there was a generation or ſeparation of oxygen air from the water; for the air in which the mice had died, did not contain a particle of that fluid; and the air in which the mint had grown for ſome time, muſt have had freſh oxygen produced in it, or it would have ſuffocated the ſecond ſet of mice in a moment.

EXPERIMENT UPON THE AIR THAT IS PRODUCED BY PEAT-MOSS, OR TURF-BOG, WITH OBSERVATIONS UPON THIS MOST SINGULAR SPECIES OF VEGETATION.

Having been called upon to attend a gentleman of fortune, whoſe houſe is ſituated at no great diſtance from an immenſe tract of peat-moſs, or turf-bog, in the Queen's county, it gave me an opportunity of becoming acquainted with ſeveral particulars reſpecting that ſubſtance, I had long wiſhed to inform myſelf about. On my firſt arrival, I conceived that the wretched tenants of the miſerable ſod-built huts, on the borders of the bog, muſt, in ſpring, when the moiſture begins to be exhaled by the heat of the ſun from off the ſurface of the bog, be ſubject to intermittents, and other diſorders which are ſuppoſed to be occaſioned by *marsh miasmata* afloat in the air; but, upon enquiry, I found that this was by no means the caſe, for the poor ſqualid bog-trotter is not more ſubject than his neighbour, who lives in more elevated and dry ſituations, to ague or other kinds of diſeaſe which the putrefaction of vegetable ſubſtances is ſaid to give birth to.

I determined to try what kind of air the ſurface of peat-moſs yields, and having procured a flat piece, I took it to Dublin with me in a deal box, for the purpoſe of examining the air I might be able to extract from it. The next day I obtained upwards of four ounce meaſures of air, from different portions of the freſh moſs, and found that it was very pure oxygen air.

Dr. Anderſon has, ſince that time, publiſhed a very ingenious and intereſting pamphlet on the nature and properties of peat-moſs, and has given ſome uſeful inſtructions for the improvement and cultivation of large tracts of boggy country. He ſays that peat-moſs is in a continued chain of vegetation from the lowermoſt to the uppermoſt ſtratum of the bog, and that while it continues to vegetate, no *living* animal has ever been found in it, nor can any other kind of vegetable be made to grow in it, till its own vegetation (on its ſurface at leaſt) be put a ſtop to. Lord Cromartie has furniſhed us with a very different account of a moſs near the town of Elgin in Murray. " Though there is (ſays his lordſhip) no river or water which communicates with the moſs, there are little ſhell-fiſh reſembling oyſters, with living fiſh in them, in great quantities, though no ſuch fiſh are found in the adjacent river, nor even in the water-pits in the moſs, but only in the ſolid ſubſtance of the moſs itſelf." See notes to Darwin's Botanic Garden, p. 36.

I have had frequent opportunities of converſing with ſeveral of the moſt experienced reclaimers of bog, and many of the oldeſt turf-cutters in Ireland; and have been informed by both, that living animals, ſuch as ſhell-fiſh, are very rarely to be met with in turf-bogs. One old gentleman, who was born on the borders of the bog of Allan in the Queen's county, aſſured me that he never heard that fiſh like an oyſter had been taken out of any turf-bog in his native county; and it contains as many acres of peat-moſs, I believe, as any county in Ireland. The diſcovery of the fiſh therefore in the moſs at Elgin, muſt be conſidered as *rather* a ſingular circumſtance.

Peat-moſs has been pretty generally thought to be produced by the decay of large tracts of foreſt; and hence it is that we ſo frequently meet with the roots, trunks, and branches of large oaks, lofty firs, and huge yew-trees, at conſiderable depths below the ſurface of bogs in Scotland and Ireland. But as there are no traces of foreſts having ever exiſted in other bogs of vaſt extent, it muſt be admitted that *all* peat-moſs is not produced by the decay of woods. For my part, I am of opinion, that if water were conſtantly to continue to flow around the roots of a particular claſs of aquatic vegetables, it would produce peat-moſs in *any* country. That widely expanded bog, which commences upon the banks of the Shannon about Laneſborough, and which branching off in many various directions according to the level of the country, covers ſo great a portion of the counties of Longford, Leitrim, Roſcommon, Weſtmeath, King's and Queen's counties, Tipperary, Galway, Kildare, &c. is kept in a ſtate of vegetation (which, according to Dr. Anderſon, is the only thing neceſſary to conſtitute peat-moſs) by the waters of the Shannon; which, prevented from finding a ready paſſage to the ſea, by the high fall at Killaloe, and other obſtructions in the river, overflowed their banks, and inundated the low grounds, which, aſſiſted by a moiſt and temperate climate, produced moſt luxuriant vegetation in the moſs at a very remote period, and will continue to promote its growth for ages yet to come, unleſs proper means can be deviſed for lowering the bed of the Shannon, by which only the bogs in that country can be drained. And without previous draining they never can be improved, draining being the *sine quâ non* of bog reclaiming.

It is unneceſſary for me to ſay any thing on modes of reclaiming bog or peat-moſs, to my countrymen, the Iriſh country gentlemen.* There are no men who underſtand the ſubject better, or practiſe it to greater extent or more advantage, than many of them do, where proper drains and levels can be made to carry off the water. They may not all perhaps know that peat-moſs is a congeries of vegetation, and that to enable other vegetables to take root and thrive in it, it is firſt neceſſary to deſtroy its own vegetative principle. But though they may not, I ſay, have made themſelves acquainted with all the facts reſpecting this curious ſubject, with that kind of accurate attention which Dr. Anderſon has beſtowed upon it, they nevertheleſs know enough, viz. that there are no circumſtances neceſſary to be attended to in reclaiming turf-bog, but deep draining, turning up the ſurface of the bog with the plough as ſoon as cattle can walk over it; and laſtly, by manuring it highly with that beſt of all calcareous manures for this purpoſe, lime-ſtone gravel, when it can be conveniently procured. After draining and working the ſurface either with the plough or ſpade, it will, even in the firſt ſeaſon, reward the huſbandman pretty amply for his trouble with a good crop of rape. The next year after the lime-ſtone gravel has been drawn out on it, or that it has been marled, limed, or covered with mould, it will produce moſt abundant crops of oats, or other grain; and after that, when laid down with graſs feed, it becomes the richeſt and moſt luxuriant meadow land in the world. It does not for a long time, however, I have obſerved, bear to be trodden npon by heavy cattle in the winter, but it affords excellent paſture for yearling calves at that ſeaſon of the year. I have enquired from many reclaimers of bog, whether it is ſafe paſture for ſheep, and have heard that in many inſtances *it is;* but as I know that in ſome others it is *not*, experience is the only thing that can direct the farmer in this particular.

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* Mr. Trench, of Wood-Lawn, County of Galway, and Mr. W. Curtis, of Anaghmore, King's-County, deſerve civic crowns for their induſtry and ſkill in reclaiming bog.

Let us now enquire into Dr. Anderſon's poſtulatum, " that no other ſpecies of plant can be made to grow in peat-moſs, till its own tendency to vegetation be firſt counteracted," with a little more attention, before we diſmiſs this part of the ſubject.

Every hour's obſervation furniſhes a traveller in Ireland with irrefragable evidence to refute the above aſſertion of the learned Doctor; for there is ſcarely a piece of bog in the iſland, on ſeveral parts of the borders of which they have not planted Scotch-fir, alder-trees, pine-poplars, and other aquatic trees, without having taken any other means of reclaiming the bog, or of putting an end to the vegetation of the moſs of which it is compoſed, than ſimple encloſing. Of this there is an inſtance in a long plantation through a bog on both ſides of the road leading to the gateway of Monaſter-Evan park, the ancient ſeat of the Earl of Drogheda; and there are ſeveral clumps of fir (not very flouriſhing ones indeed) on many patches of bog in the neighbourhood of Amoe-park, the modern princely reſidence of the Earl of Port-Arlington. To do every juſtice however to the accuracy of Dr. Anderſon's obſervation, I am free to admit that theſe trees are very ſcrubby very ſtunted, and very unhealthy in their appearance. Their roots, it may be, juſt draw nouriſhment ſufficient, from among the parts of the moſs which are beginning to decay and to be changed into mould, (in conſequence merely of the drain or ſhallow ditch of the incloſure) to keep them in a ſtate of languiſhing exiſtence; but there is not yet enough of the living moſs converted into earth, to give vigour or verdure to the trees.

ENQUIRY WHETHER THE FLAVOUR OF FRUIT IS DERIVED FROM THE OXYGEN THAT IS CONTINUALLY STRAINING THROUGH THE LEAVES OF TREES.

Having obſerved that gooſeberries, which continue to grow on trees after they have been deprived of their leaves by the voracity of caterpillars early in the ſummer, are almoſt wholly deſtitute of the taſte peculiar to this kind of fruit; it occurred to me that the flavour of every ſpecies of fruit is derived from the oxygen, or acidifying principle, which the leaves of all plants is unceaſingly imbibing and emitting, during the time they are expoſed to the influence of light. It was an eaſy matter to put the conjecture to the teſt of an experiment; I did ſo, and in the ſummer of 1792, I pulled every leaf off a few gooſeberry, raſberry, and dwarf apple-trees, leaving the fruit on; the effect of which was, that the fruit of the gooſeberries and raſberries which had nearly arrived at half its natural ſize when the experiment commenced, never attained its full growth, or acquired the diſtinguiſhing taſte of the reſpective fruits; and as to the apples, they almoſt all dropped off the branches in a ſhort time after they had been deſpoiled of their leaves; the few that remained on the trees till towards the middle of Auguſt, were ſtunted in their growth, withered and ſhrivelled in their appearance, and almoſt entirely devoid of juice and flavour.

Within theſe few months, a moſt ingenious and uſeful little eſſay on the culture of apple and pear-trees, by T. A. Knight, eſq; of Herefordſhire, has fallen into my hands; on peruſing it, I find that a ſomewhat ſimilar idea to mine has ſuggeſted itſelf to that very obſervant and intelligent gentleman; for he ſays, "The leaves of trees are the laboratories in which their juices are prepared from the oxygen that is continually paſſing through them; and he likewiſe obſerves, that if the leaves of a branch of a pear, which had but a few years before been engrafted upon an old tree, happen to fall or be pulled off, the fruit of that bough will never come to perfection, but will very ſoon wither and fall off alſo."

From the above facts, it is very obvious that the agreeable ſubacid flavour of all fruit is derived from the oxygen, which, during the day time, is conſtantly circulating through the leaves of trees, and paſſing from them through the ſap-veſſels into the fruit.

OBSERVATIONS ON THE AIR PRODUCED BY A PECULIAR KIND OF GREEN VEGETABLE SUBSTANCE WHICH PROPAGATES ITSELF SPONTANEOUSLY IN STAGNANT WATER; TO WHICH IS ADDED, AN ENQUIRY WHETHER OXYGEN AIR IS ELABORATED BY LEAVES GROWING IN LIGHT, IS SECRETED FROM THE ATMOSPHERE, OR PRODUCED BY THE DECOMPOSITION OF WATER.

It has been proved by Dr. Prieſtley that there is no vegetable production which yields ſo much vital air as a certain green ſubſtance (ſuppoſed to be vegetable) which is univerſally to be met with in water that has been ſuffered to ſtagnate for eight or ten days. The Doctor does not mean that vegetable which is ſo univerſally to be met with on the ſurface of ſtagnant pools and ditches, called ducks'-meat, (*lemna minor*) but a much more minute ſpecies of vegetable, which may frequently be obſerved to adhere to the inſides of the fountains of bird's-cages, and which will be ſpontaneouſly produced in a ſhort time in the pureſt water that is left to ſtagnate in any veſſel expoſed to the influence of light. I have made a number of experiments with this remarkable plant, and have ſatisfied myſelf, that it actually does either ſtrain from the air, or ſeparate from the water, by decompoſing it, a much larger quantity of oxygen air than can be obtained from a much greater proportion of the leaves of any other plant I have made the ſubjects of my experiment. I have alſo proved, that like all other plants it continues green no longer than while, by the aſſiſtance of light, it is generating vital air; for if the veſſel in which it is growing be ſecluded from light for ten days or a fortnight, it is deprived of its green colour, and with it loſes the power of producing pure air: on the contrary, when it becomes white, in conſequence of the excluſion of light, it yields azotic gas only.

Having hinted, in this ſection, that the production of vital air by the leaves of plants growing in light, has been attempted to be accounted for in different ways—by elaboration, by ſecretion from the atmoſphere, and by the decompoſition of water; I ſhall enter a little more minutely into this part of my ſubject in this ſection than I have done in a former one, in which I have firſt introduced the ſubject to the reader.

In the ſection alluded to, I ſtated that the whole of the atmoſphere is ſuppoſed to be imbibed by the leaves of all vegetables growing in light, and that the impure part is by ſome innate power of the plants applied to their nouriſhment and ſupport, while the vital air is rejected as a ſubſtance totally incapable of promoting vegetation, and is therefore thrown back into the common ſtock of air. This theory was given to the world in a letter from Sir Benjamin Thompſon (now Count Rumford) to Sir Joſeph Banks. It entirely overturns the doctrine of Dr. Ingenhouſz, who is of opinion that pure air is *elaborated* or *generated* by leaves growing in the light; according to this hypotheſis, there is no freſh production, but ſimply a ſecretion of vital air that previouſly exiſted in the atmoſphere, by the leaves of plants when expoſed to the action of light. Fourcroy, a very celebrated French chemiſt of the preſent day, differs from both theſe philoſophers, and thinks that leaves neither *elaborate* vital air according to the idea of Ingenhouſz, nor as Sir B. Thompſon ſuppoſes, ſtrain it from the atmoſphere, but produce it by decompoſing water. "Hitherto (ſays this incomparable philoſophical chemiſt) the art of chemiſtry has arrived at the knowledge of no means of decompoſing water, but by combuſtible ſubſtances, which take from it its oxygen: we are unacquainted with any capable of attracting its hydrogen and ſetting its oxygen free. It would ſeem, however, that nature has inſtruments for effecting this inverſe manner of decompoſing water: the leaves of vegetables, ſtruck by the rays of the ſun, appear to decompoſe water, by abſorbing its hydrogen and diſengaging its oxygen in the form of vital air. This we may preſume to be in part the mechaniſm of vegetation, of the formation of oils, and of the renovation of the atmoſphere." See Fourcroy's Philoſophy of Chemiſtry, p. 38.

As theſe three different theories agree in this point, that the leaves of plants growing in light purify or renovate the air, by ſupplying it with freſh oxygen, it is a matter of no ſort of conſequence to the inferences I mean to draw from the fact, how it is done, whether according to Ingenhouſz's, Thompſon's, or Fourcroy's hypotheſis; however, as it is a matter of curious ſpeculation, let us ſtop a few minutes to enquire which is the moſt probable theory. The experiment of Dr. Prieſtley above recited affords very good ground to commence the inveſtigation upon. The Doctor, to reſtore air in which mice had died to purity, put a ſprig of mint to grow in the jar containing the air, and in a ſhort time found that it purified it ſo much that other mice lived as long in it as the firſt had done. Here it muſt be admitted there *might* have been a generation of vital air by the leaves of the mint, as Ingenhouſz imagines is uniformly the caſe; but it is not very probable that ſo ſmall a ſprig of mint could have elaborated ſo much oxygen as was neceſſary to reſtore the contaminated air to purity in ſo ſhort a time as we are told it was effected. Vital air could not have been ſtrained from the air within the jar, becauſe all that had been originally there was exhauſted by the mice, or they could not have been ſuffocated. *Water* there was in the jar, from which the oxygen might have been detached by the united agency of light and the vegetable, which aſcending into the jar was capable of ſupplying the place of that which had been conſumed by the reſpiration of the incloſed animals. This, and ſome other circumſtances hereafter to be adverted to, ſeem to corroborate the idea that the leaves of plants growing in light renovate the air by no other means than the decompoſition of water. It may be aſked, where is the water in the atmoſphere? To this I anſwer, there is always enough in ſolution in the air, even in the dryeſt weather, fully to anſwer the purpoſe.

Another proof that the oxygen is derived from the decompoſition of water is, that it has been diſcovered by Prieſtley, that the leaves of willows and other aquatic plants produce more vital air than an equal weight of the leaves of trees that do not like a ſwampy ſoil to grow in. I have myſelf proved, by experiment, that the leaves of a particular willow, (*ſalix viminalis*) when it grows in the moiſt earth beſt ſuited to it, gives out more pure air than the leaves of the ſame ſpecies of plant which has been cultivated in higher and dryer ſituations. This ſeems to be a further proof that leaves abſorb oxygen from water and not from the air. Dr. Ingenhouſz, in the fourth ſection and twenty-ſecond page of his Experiments upon Vegetables, adduces an experiment backed by an argument, to convince us "that the air oozing out of the leaves in water, is not from the water itſelf;" but his experiment and argument are totally irrelevant to the queſtion now before us. The Doctor ſays, that the air diſengaged from water, without the intermediation of a green vegetable and light, is worſe than common air. Fourcroy expreſsly declares that the oxygen of water *cannot* be ſeparated from the other component part, hydrogen, except by the interpoſition of leaves and light; Ingenhouſz's argument, therefore, that the air collected from water without the agency of a vegetable and light is worſe than common air, is of no weight whatever in the preſent enquiry.

OBSERVATIONS ON PLANTATIONS IN GREAT DOMAINS, AND ON THE CHOICE OF PLACES TO EXERCISE IN, AT DIFFERENT TIMES OF THE DAY.

HAVING enquired into the various ways in which the vegetable creation is ſuppoſed to renovate and perpetuate the purity of the atmoſphere, let us endeavour to turn our acquaintance with this truly intereſting proceſs to ſome good practical purpoſes; firſt, in furniſhing country gentlemen with hints that may enable them to plant more judiciouſly than they do at preſent, about their houſes, offices, farm-yards, &c.; and ſecondly, in pointing out to invalids of ſeveral deſcriptions, proper ſituations for them to exerciſe in, at different times of the day and year.

It has been already mentioned, that ſome kind of trees generate oxygen in a much larger proportion, in a given time, than others do. All the ſpecies of willow, of poplar, and indeed of all aquatic trees, produce, I am induced to think from the experiments I have made, at leaſt an eighth more vital air, in the courſe of the day, (I mean from the riſing to the ſetting of the ſun) than any other trees we are acquainted with. Next to theſe come trees with the moſt vivid green leaves; ſuch as limes, horſe-cheſnuts, the oriental platanus, and elms. Next to theſe are the firmer and darker-leaved deciduous trees, oaks and beech; and after them follow evergreens of every kind: but as the leaves of every ſpecies of theſe laſt yield the ſame quantity of pure air in winter that they do in ſummer, no plantation ſhould be without a proper proportion of firs of every kind, of arbutus trees, Portugal laurels, common ditto, lauruſtinuſes, hollies, phillaræas, ivy, &c. &c.

As the reſpiration of animals, the fermenting and rotting of vegetables, the putrefaction of animal matter, and the burning of lime, are all proceſſes which deſtroy the purity of the atmoſphere; gentlemen ſhould make pretty large plantations of ſuch trees as are known to produce oxygen in the greateſt abundance, at proper diſtances from their ſtables, cow-houſes, and ox-ſtalls, and from the places where large heaps of dung from the farm-yard are uſually depoſited to ferment and rot, and from lime-kilns.

Theſe plantations ſhould, if practicable, entirely ſurround the farm-yard, and be from one hundred and fifty to two hundred yards diſtant from it; that the oxygen prepared by trees at ſuch a diſtance may be brought by the wind, from whatever point it blows, to ſupply the place of the air that is undergoing continual contamination from the reſpiration of a number of animals, the putrefaction of a great variety of animal and vegetable ſubſtances, the burning of lime, &c. which would contribute very conſiderably towards the preſervation of the health of the animals. What a prodigious number of fine horſes have I ſeen afflicted with convulſions, fevers, inflammations on the lungs, and ſeveral other acute diſeaſes, in conſequence of their having been ſhut up in confined ſtables, perhaps with a heap of their own dung fermenting at their heels.

Such plantations adjoining to every great kennel would alſo be the means of keeping the hounds, in ſummer, from madneſs, and many other diſeaſes. When the Duke of Portland was Lord Lieutenant of Ireland, in 1783, I heard that in a large kennel of his Grace's, at his ſeat at Welbeck, built in looſe ſand, in a fine open unconfined ſloping ſituation, well ſupplied with water, and every thing that was thought neceſſary to tend to the health of the hounds, there was a particular yard ſet apart for the breeding bitches; in which, though every care was taken to render it as convenient for the creatures as poſſible, I was aſſured, they could never rear a ſingle puppy. They all died while very young of fits; but if the bitches were removed a little before their pupping time, to the ſtables, or other out-offices adjoining to the dwelling-houſe, they hardly ever loſt a whelp. This could have ariſen from no other cauſe than the contamination of the air of the yard, by the reſpiration of the great number of dogs in the other parts of the kennel. The ſupply of oxygen was not equal to the conſumption of it; if it had, the mortality among the puppies would probably have ceaſed.

All plantations in great demeſnes ſhould be at ſuch a diſtance from the dwelling-houſe, as that the oxygen which the leaves are pouring out during day-light, and the azote they are parting with in the night-time, ſhould be very well mixed with the ſurrounding intermediate air, before it finds its way into the apartments. Plantations very cloſe to the windows of a houſe are exceedingly ill-judged, becauſe, from ſun-riſe to ſun-ſet, they are throwing a large quantity of oxygen, undiluted by any other kind of air, into the chambers, which may be highly injurious to ſeveral individuals in the family; and as, from ſun-ſet to ſun-riſe they produce the moſt impure vapours only, they cannot fail, during that time, of being noxious to every perſon in the houſe. The cuſtom, therefore, of making ſuch plantations under the windows of almoſt every houſe in England and Ireland ſhould be diſcontinued. The practice of bringing a great number of pots of hot-houſe and green-houſe plants into drawing-rooms and parlours, ſhould, for the ſame reaſons, be laid aſide alſo.

Surrounding great demeſnes with extenſive plantations of all kinds of foreſt-trees and evergreen ſhrubs, is not only very ornamental, but at the ſame time exceſſively beneficial; for ſuch ſcreening plantations afford ſhelter, and furniſh a continual current of vital air, which muſt be wafted into the grounds, let the wind blow from what point of the compaſs it may. The great maſs of planting in every extenſive demeſne ſhould be to the weſt and ſouth-weſt of the houſe; becauſe winds from thoſe points are much the moſt prevalent in England and Ireland; the vital air, therefore, from plantations in theſe directions, will, for the greater part of the year, be conſtantly flowing towards the houſe and offices, where there is the greateſt conſumption of it.

It is now very generally admitted, that in a great multiplicity of diſeaſes, there is too ſmall a proportion of oxygen in the ſyſtem; while, on the contrary, in ſome others, there is a ſuperabundance of that principle. The caſes in which there is a defect of vital energy, are much more numerous than thoſe in which there is an exceſs of it. Among the former we may include all the orders, genera, and ſpecies of Dr. Cullen's two claſſes, Neuroſis and Cachexiæ; viz. palſy, dyſpepſia, hypochondriaſis, melancholia, ſpaſmodic affections, palpitations, (not proceeding from organic affection of the heart) aſthma, diabetes, hyſteria, tabes, anaſarca, aſcites, hydrothorax, ſcorbutus, chloroſis; to which may be added, atonic gout, and chronic rheumatiſm, (eſpecially in old ſubjects) ſchirrhus, lethargy, jaundice, ill-conditioned ulcers, caſes of great debility after every ſpecies of typhus-fever, leucorrhœa, &c. The diſorders in which further oxygenation is unneceſſary, and which it might be dangerous, are acute and inflammatory; but, as in all complaints of this deſcription, (except the earlier ſtages of florid conſumption) the patients are for the moſt part unable to uſe exerciſe in the open air, I ſhall confine my obſervations, where hyper-oxygenation is preſent, entirely to this laſt complaint.

In every diſeaſe in which there is too little oxygen in the ſyſtem, the ſick ſhould endeavour to acquire a ſufficient quantity of that ſo indiſpenſably neceſſary principle to health, by every means that can be deviſed, conſiſtent with ſafety. The lungs are the moſt natural route by which any ſubſtance in its aerial form can be conveyed into the ſyſtem. The inſpiration, therefore, of air of an higher ſtandard than the atmoſphere, is now recommended by ſeveral very able phyſicians; and there are a great number of well-authenticated caſes of exceedingly obſtinate nervous affections of various kinds; even when they have verged towards melancholia, and of other very refractory complaints, in which the practice has been attended with the moſt perfect ſucceſs.* Perſons afflicted with ſuch maladies ſhould not neglect even the ſmall additional quantity of vital air, which the neighbourhood of woods, groves, or hedge-rows, affords during the day-time; they ſhould, therefore, when they go out in a carriage, on horſeback, or on foot, make choice of the ſheltered, and if poſſible, the ſunny ſide of extenſive plantations, to take their exerciſe in about one o'clock; for it has been proved by Doctor Ingenhouſz, that the leaves of trees pour forth the pureſt air after the ſun has paſſed the meridian. (See Exper. upon Vegetables, page 44.) Evergreens, the lauro-ceraſus in particular, do not begin to produce pure air till late in the day. (Exper. on Vegetables, p. 223.)

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* See Townſhend's Guide to Health; and a Collection of Caſes lately publiſhed by a Society of Phyſicians in London.

It would alſo, I am ſatiſied from one or two caſes that have fallen under my obſervation, be attended with the moſt ſalutary conſequences, if valetudinarians who are ill from a deficiency of oxygen (in whoſe caſes exceſſive debility does not prevail) were to ſpend the greater part of the middle of every day, in ſummer and autumn, in riding and walking about woods and groves; or in reading, converſation, playing at ſhuttlecock, billiards, or in attending to muſic in temples, green-houſes, moſs-houſes, dry well-aired grottos, or ſuch buildings as are common in ſhrubberies and wooded ſcenes, where the trees produce much purer air than is to be met with in more open ſituations.

Invalids of this deſcription ſhould avoid all great aſſemblies, ſuch as balls, routs, &c. at which vital air is conſumed by the reſpiration of the company and the combuſtion of the candles, a great deal faſter than freſh can find its way into our preſent faſhionable apartments, from which modern refinement in luxury has as ſtudiouſly ſhut out the free acceſs of air, as if it were noxious, inſtead of being neceſſary to animal life; and they ſhould ſpend their afternoons and ſleep in ſpacious and well-ventilated chambers, the windows of which ſhould not look into a ſhrubbery, from which azote inſtead of oxygen is exhaling during the whole courſe of the night.

As there is a ſcarcity of oxygen in many of the diſeaſes to which children are incident, all ſuch as are ricketty, badly nurſed, pot-bellied, or diſpoſed to hydrocephalus internus, or water on the brain, ſhould paſs the greater part of the middle of the day in fine weather in the nurſe's arms, or at play, (according to their age, and other circumſtances) in the neighbourhood of ſhrubberies or more extenſive plantations; but when children are hectic, let them avoid wooded ſcenes, and take air and exerciſe in large open fields, or upon uncultivated commons; for the ſmalleſt additional oxygenation of their blood may be highly injurious to them.

But as the atmoſphere alone cannot furniſh as much pure air as may be required in the great variety of diſeaſes in which it is deficient, it has been found neceſſary to have recourſe to medicines that contain a great abundance of oxygen, for which their attraction is ſo weak, that they part with it very freely—ſuch as the nitric and oxygenated muriatic acids, and the oxygenated muriate of pot-aſh; the ſucceſs of which, in the few trials that have been made, has been ſo great, that there is reaſon to hope theſe new remedies may be found to be a moſt valuable acquiſition in the practice of medicine. Some ſpeculative phyſicians* are ſo prepoſſeſſed with this idea, as to imagine that the ſanative effects of every mercurial preparation, in all the diſorders in which they are uſed, and particularly in that for which they are the grand ſpecific, may be owing to the diſengagement of their combined oxygen; and hence it is, probably, that other ſubſtances ſurcharged with it have been brought into practice as a cure for this complaint.

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* Dr. Girtanner of Vienna, Dr. Swediaur, and Dr. Buchan the younger.

Mr. Scott, a ſurgeon at Bombay, is ſo ſanguine as to aſſert that nitric acid is equal if not ſuperior to mercury, as an antiſyphilictic remedy. That it is very efficacious in many ſtages of the complaint, has been moſt inconteſtibly proved in the communications of Dr. Beddoes and Mr. Cruikſhank, and under my own obſervation at the Lock Hoſpital in Dublin. I confeſs, however, that I am one among a number of practitioners who think it a very fortunate circumſtance for mankind, that we have ſtill the old ſpecific to reſort to. The acid is indiſputably a moſt powerful auxiliary medicine, and as it is not injurious to the conſtitution, it may be right to let it precede mercury in delicate habits; but in my opinion it will never ſuperſede that metal in the cure of any diſeaſe for which it has for ages been eſteemed a ſovereign remedy.

To the long catalogue of complaints in which there is a ſcarcity of oxygen in the ſyſtem, apparent death from drowning, ſtrangulation, and ſuffocation, may be added. We have already ſeen that Dr. Goodwyn has endeavoured to prove that life is ſuſpended, in all ſuch caſes, ſolely in conſequence of the blood not meeting with vital air in the lungs; which is followed by ſyncope, a livid colour of the ſkin, and an accumulation of *black* blood at the left ſide of the heart. Mr. Coleman, the profeſſor of veterinary medicine in London, controverts the theory of Dr. Goodwyn with reſpect to the *immediate* cauſe of death in drowning, &c. and contends for it, that the oxygen taken in by the lungs in reſpiration is of no other uſe than to ſupport animal heat, and by the maintenance of animal heat, to keep up animal irritability. The collapſe of the lungs, and *not* the accumulation of *black* blood at the left ſide of the heart, is the cauſe of the ceſſation of the circulation in every caſe of ſuffocation, &c. in the opinion of this author. (See Coleman on Suſpended Animation, p. 47 and 147.)

It is moſt probable that the lungs *do* collapſe, in all ſuch caſes, before the heart ceaſes to vibrate; but, as an accumulation of *black* blood in the left auricle and ventricle, and a collapſe of the lungs, are *both* to be met with where reſpiration has been diſcontinued but for a few minutes in every caſe of ſuffocation, &c. it is a matter of very little importance to determine to which it is that we are to aſcribe the *immediate* cauſe of the ſuſpenſion of the vital functions. The means to be reſorted to for the recovery of the perſon apparently dead, muſt be the ſame, let the interruption of the circulation proceed from whichſoever cauſe it may.

Theſe means may be compriſed within a very narrow compaſs; for the art of reſuſcitation depends entirely upon proper attention to the three following circumſtances; inflating the lungs with as pure air as poſſible, ſtimulating the heart to renew its regular contractions and dilatations, and reſtoring the body to its natural temperature.

The inflation of the lungs is generally performed either with a common pair of bellows, or with the double bellows of the late ſo juſtly celebrated John Hunter. The firſt is a very clumſy inſtrument for this purpoſe, for with it we can do no more than ſimply diſtend the lungs; but to force the air out of them again, we are obliged to withdraw the noſe of the bellows, and preſs upon the cheſt. To remedy this defect, Mr. Hunter invented an inſtrument on the principle of a pair of bellows, that is, ſo conſtructed as to inflate and exhauſt the lungs alternately, ſo as, in ſome meaſure, to imitate natural reſpiration; but it is by no means a perfect inſtrument. Oiled ſilk bags, capable of containing from twenty to thirty quarts of air each, with a flexible tube affixed to one corner, to be inſerted under the epiglottis into the trachea, is infinitely preferable to any contrivance of the bellows kind; becauſe, with ſuch an apparatus, air containing more oxygen than the atmoſphere can be thrown into the lungs, (for the purpoſe of preparing which every medical aſſiſtant to the Humane Society ſhould be furniſhed with proper retorts and glaſs jars) and becauſe the gentle preſſure that is required to force the air out of ſuch bags cannot produce ſo great a degree of friction at the place where the tube is inſerted, as the working of a pair of bellows often does. I have heard of an inſtance of a recovery from ſuffocation, in which this friction, and the proceſs of rubbing the ſurface of the body with ſalt, was ſucceeded by ſuch an excoriation at the noſtrils, and ſo great irritation all over the ſkin, as were exceedingly inconvenient to the patient for a conſiderable time after he was reſtored to life.

The beſt manner of ſtimulating the heart to renew its action is by paſſing ſlight ſhocks of electricity through the thorax in different directions; obſerving not to diſcharge the Leydon phial till we are certain the lungs are ſufficiently diſtended with air to remove compreſſion from the pulmonary artery, and open a paſſage for the blood to get within the reanimating influence of oxygen, which it cannot poſſibly do as long as the organs of reſpiration continue collapſed and flaccid.

In ſome of the papers of inſtructions for the recovery of perſons apparently dead, which were firſt diſtributed by the Humane Society, blowing the breath of an healthy man into the mouth of the perſon about to be recovered, was recommended as one means of inflating the lungs; but this was very reprehenſible practice, for as the air expired by all animals is fixed air, ſuch a procedure was more calculated to extinguiſh than to rekindle the latent ſpark of life.

Various ſubſtances have been in uſe for reſtoring the temperature of the bodies of the apparently dead:—Warm grains from a brewery or diſtillery, fine aſhes from a glaſs-houſe, heated ſand or ſalt, cloths wrung out of warm water, &c. Water is indiſputably the beſt medium through which heat or cold can be applied to the human body, while it poſſeſſes the power of reaction; but when that power is ſuſpended, it is liable to the following objections: The evaporation of fluids generates cold; a very conſiderable evaporation always takes place from the ſurface of our bodies after coming out of a warm bath, which is ſucceeded by a ſlight degree of ſhivering, or a chilly ſenſation, which laſts for a longer or ſhorter time, according to the degree of reaction of the ſyſtem that may be required to overcome it. Warmth that is ſucceeded by ſuch effects muſt be obviouſly improper, when the vital powers (eſpecially that of generating heat) are lying dormant. Dry ſubſtances, ſuch as ſand or ſalt, heated to about ninety-five or ninety-ſix degrees of Farenheit's thermometer, is infinitely preferable to warm water in caſes of apparent death. I have found fine ſand, which is to be had almoſt every where, and which can be very expeditiouſly heated in warming-pans, frying-pans, iron-pots, &c. in great quantities, a moſt convenient ſubſtance. A part of it ſhould be ſpread under the body, and the reſt applied in pillow-caſes, or other ſmall bags to the ſides and abdomen.

It has been, and with many of the medical aſſiſtants to Humane Societies I believe ſtill is, a very common practice to throw the ſmoke of tobacco up the rectum, by means of an appropriate apparatus, with the double view, perhaps, of reſtoring heat, and producing ſtimulus; but this is a very injudicious procedure, for tobacco occaſions fainting, giddineſs, and nauſea, in many people who are not accuſtomed to the uſe of the plant, and thoſe effects, inſtead of rouſing the latent ſpark of life, may totally extinguiſh it; and as for ſtimulus, that ſhould be applied to the thoracic and not to the abdominal viſcera.

Friction is a very uſeful part of the proceſs for the recovery of perſons apparently dead, but it ſhould not be performed with ſalt, ſand, or other rough ſubſtance, becauſe I have already ſhewn that they produce excoriation and great irritation after the perſon is reſtored to life. The fleſh-bruſh, warm flannels, and coarſe cloths, are not ſo objectionable; but the warm hand moiſtened a little with oil, to render the friction more eaſy, is far preferable to any other mode of aſſiſting the aſcent of the blood through the veins towards the heart, which is one of the principal reaſons for having recourſe to the rubbing of the ſurface of the body—the motion of the hand ſhould therefore be upwards only.

I ſhall conclude this part of my ſubject with one general remark, viz. that the proceſs ſhould be conducted in as airy a ſituation as poſſible. In warm and dry weather it will be adviſeable to enter upon it in the open air; but when we are obliged to bring the body into a houſe, we ſhould throw up the windows, and permit no more perſons to enter the room than are abſolutely neceſſary to carry the directions of the medical aſſiſtant into execution.

Having diſmiſſed the conſideration of caſes that require a ſupply of oxygen, I am to proceed to treat of a complaint in which it is redundant,—*Florid* Conſumption.

The diſtinctive epithet *florid* has been applied to this ſpecies of the diſeaſe, becauſe that during its approach, and towards its commencement, we perceive, at the time of the acceſſions of feveriſhneſs, a vivid redneſs of the cheeks, and an extraordinary redneſs of the lips, tongue, and fauces; and becauſe that blood diſcharged from the noſe, lungs, or any part of the body, has a colour evidently more florid than uſual. A much greater acuteneſs of the ſenſes is alſo obſervable in this diſeaſe, juſt before the period of extreme debility and emaciation comes on; and, in the indiſpoſition preparatory to conſumption, a remarkable ſymptom occurs much more frequently than is commonly underſtood. This ſymptom is a convulſive cough, often of ſuch alarming violence as to be likely to perplex and diſconcert inexperienced practitioners. It ſeems to alternate and intermix with the well-known ſhort dry cough, which is ſo invariably attendant upon the diſorder. See Dr. Beddoes's letter to Dr. Darwin, p. 18.

Among the variety of new and ingenious ideas which Dr. Beddoes entertains concerning this ſpecies of conſumption, the following is particularly entitled to the attention of the medical philoſopher.

"It is an evident conſequence of my leading opi-
"nion, that a phthiſical patient would take longer
"time than another perſon in being drowned, or in
"being ſuffocated in moſt of thoſe airs that are
"unfit for reſpiration; I ſay *moſt*, becauſe I ſuſpect
"that nitrous air might furniſh an exception. Being
"already provided, as I ſuppoſe them to be, with a
"larger proportion of that principle which reſpira-
"tion introduces into the body, they muſt be able
"to continue a longer time without the neceſſity of
"a new ſupply; and as the left cavities of the heart
"ſeem to be more irritable in ſuch patients, it is
"probable that they would be more eaſily recover-
"able from accidents of this kind, than perſons in
"an ordinary ſtate of health." Beddoes's letter to Dr. Darwin, p. 10.

Conſumption is moſt unqueſtionably more prevalent among the upper and middle ranks in England and Ireland, than it was formerly. How are we to account for this? I am induced to aſcribe it to the following cauſes;—the difference between the manner of living, habits, dreſs, &c. of the preſent race and their anceſtors; and to the change which the draining, planting, and other agricultural improvements, has made in the air; to theſe may be added the great diſſimilarity in the conſtruction, ventilation, furniſhing, &c. of our houſes. Dr. Beddoes attributes it ſolely to the two latter cauſes. "The inhabitants "of this country, (ſays he) almoſt without exception, "breathe a freer air than their anceſtors; nor do I "believe there is any particular in which the difference between the preſent and ſome paſt generations is ſo remarkable. We ſee then, that the "ſubjects of our Edwards, our Henrys, and of good "Queen Beſs, may have found (in being more free "from ſo formidable a diſeaſe than their delicate "and airy poſterity) ſome compenſation for the "confined air and filth in which they paſſed their "exiſtence." Letter to Darwin, p. 21.

As the ſtate of the ſyſtem in this diſeaſe, is directly the reverſe of what it is in thoſe I have diſcuſſed, it is obvious that the treatment muſt in every reſpect be totally different. In the former complaints, I have ſaid, we are to uſe our utmoſt endeavours to throw oxygen into the ſyſtem; in this we are to prevent the acquiſition of too great a proportion of that principle by every means in our power, conſiſtent with the ſafety of the patient. All medicines and aliments which contain a large ſhare of vital air, which they are diſpoſed to part with freely, are to be abſtained from; and all ſuch as poſſeſs but little of that elaſtic fluid are to be liberally adminiſtered. And when perſons threatened with *florid* conſumption, or labouring under it, take exerciſe, it ſhould be in open grounds, or on roads as remote from wooded ſcenes as poſſible; but as trees give out air which is peculiarly adapted to their complaint during the abſence of light, they ſhould ſleep in farm-houſes, amidſt groves, or in chambers that have windows which open into ſhrubberies, or that look into a yard in which a number of cattle of various kinds are in their reſpective ſtalls; for the oxygen taken in by inſpiration, meeting with carbon in the lungs, is changed to carbonic acid, or fixed air; a proper mixture of which with common air has been found to be exceedingly beneficial to conſumptive patients.

The ſagacious Doctor Perceval of Mancheſter, having obſerved the good effects which the application of fermenting poultices, and other ſubſtances, in the very act of generating fixed air, produce on external ſores, was induced to try whether inhaling it might not be advantageous when the lungs were in a ſtate of ulceration; and having found that it improved the appearance of the expectoration very conſiderably, he ordered a few conſumptive patients to breathe pure undiluted fixed air very cautiouſly ſeveral times in the day; but as inſpiring it, unmixed with a ſmall quantity of either oxygenous-gas, or atmoſpheric air, for ever ſo ſhort a time, is a very

hazardous

hazardous experiment, the practice never became general. I am inclined to think, however, that the reſult of the few trials which were made at Mancheſter, ſuggeſted to Doctor Beddoes the idea of mixing noxious vapours and reſpirable air together, in ſuch portions as to admit of their being breathed with impunity, by which the hyper-oxygenation of the ſyſtem is moſt expeditiouſly taken off, the feveriſhneſs abated, the dyſpnœa relieved, the expectoration improved; in ſhort, the grand obſtacles to the cure of the diſeaſe leſſened, or it may be (if the plan be entered upon before the diſorder is too far advanced) totally removed.

The effects of breathing ſuch a mixture of airs are almoſt inſtantaneous. In a letter from Mr. Crump, a ſurgeon at Albrighton in Shropſhire, to Doctor Beddoes, giving an account of the effects of a courſe of the Doctor's hydro-carbonate on his ſon, we meet with the following paſſage: "I cannot in "juſtice to you do otherwiſe than inform you that "my boy's dyſpnœa, fever, and expectoration of "matter, are always relieved by two or three inſpi-"rations; and more ſo than by any medicine I ever "gave him, or ever ſaw adminiſtered to others la-"bouring under the ſame complaints."—Letter to Darwin, p. 35.

The reſpiration of Dr. Beddoes's artificial air ſhould be repeated twice a day, or oftener, according to the urgency of the ſymptoms; and during the intervals, I would recommend the patient to ſit in a room with a number of flower-pots filled with various flowers (but particularly with the white-lily and honey-ſuckle) on tables, which will tend to keep down the oxygenation of the air. White lilies, we are told by Heiſter, yield noxious air very abundantly, and “ if a few flowers of honey-ſuckle “ *(lonicera periclymenum)* which poſſeſs the moſt “ agreeable ſmell, are able to foul, in three hours' “ time in the middle of the day, a body of air equal “ to two pints, we may judge what effect might be “ expected from a large quantity in a ſmall room;” (ſee Ingenhouſz's Exper. p. 57.) and when flowers are not in ſeaſon, fruit or roots (particularly potatoes and turnips) will be found a very good ſubſtitute; “ for all roots, when left out of the ground, “ yield by day and by night foul air, and infect the “ ſurrounding air.” Ingenhouſz, p. 53.

It has appeared extraordinary to ſeveral with whom I have converſed on this ſubject, (as I dare ſay it will to many who may read this eſſay) that breathing air leſs pure than the atmoſphere ſhould be found ſalubrious in a diſeaſe of the reſpiratory organs; and yet it has been attended with ſuch decided advantage in ſo many caſes, that the fact is moſt unqueſtionable. I have never had an opportunity of ſeeing it tried in more than one caſe, in which its effects in alleviating ſome very diſtreſſing ſymptoms were ſo immediately perceptible, that I do not heſitate to pronounce it a moſt valuable acquiſition to (I was going to ſay the *materia medica*) the *Ars medendi.*

The ſubject of the caſe was the ruin of a once athletic Iriſh country gentleman, of about forty-two years of age, whoſe mornings had, for the greater part of his life, been moſt actively engaged in the ſalubrious employment of cultivating and improving an extenſive domain; and whoſe evenings, like thoſe of the generality of his countrymen, were, for the moſt part, no leſs ſeduloully devoted to conviviality and claret. His original ſtamina, however, aſſiſted by air and ſtrong exerciſe, ſeemed for many years to ſet his occaſional intemperance at defiance. He enjoyed moſt uninterrupted good health till Chriſtmas 1796, when, in the execution of the duties of a good citizen, in forwarding expreſſes to Government at the time the French fleet was in Bantry-Bay, he caught a ſevere cold. His ſymptoms were productive of ſo little inconvenience at firſt, that he ſeemed wholly to diſregard them; he went as uſual every morning to viſit his bullocks, and in the afternoon he was, according to cuſtom, unremitting in the circulation of the bottle. A ſhort dry cough and dyſpnœa became confirmed, and were attended by feveriſhneſs, loſs of appetite and reſt, languor, and emaciation.

Having never known what illneſs was before, he was exceedingly averſe to phyſicians and phyſick, he could not therefore, for a long time, be prevailed upon by his friends to have any thing to do with either; but finding himſelf.getting worſe daily, he, at the end of ſix or ſeven months, conſented to try the effects of Briſtol water. Being at Clifton, for the recovery of my own health, at the time of his arrival, he ſent for me; and in addition to the above detail of ſymptoms, told me that he had caught cold on his journey from Ireland, which had exaſperated his complaints very conſiderably. His face was much fluſhed, his pulſe hard and very frequent, (from 110 to 115) his breathing was exceedingly laborious, his cough hard and dry, and he complained of very acute pain in the right ſide; in ſhort, he had a ſevere attack of peripneumonia.

I blooded him very copiouſly, opened his bowels, bliſtered the pained ſide, ordered a ſoft linctus for his cough, and ſome antimonial medicines to be taken after ſtated intervals, and enjoined the ſtricteſt adherence to the whole of the antiphlogiſtic regimen. After a ſecond plentiful blood-letting, and ſteady perſeverance in the above-mentioned plan for ſix or eight days, he was reſtored to pretty nearly the ſtate of health he had been in at the time he left Ireland, except that he was much reduced in ſtrength and fleſh, in conſequence of his recent attack, and the means I had been obliged to employ to combat it, As he amended, he became refractory, and being come to a place where he conceived water alone could perform miracles, he diſcarded me, and would take no more medicines. He went out upon the downs every day in his carriage, drank the water, lived as I had directed him, upon vegetables and milk, particularly cream juſt as it is beginning to ſeparate into butter and milk in the proceſs of churning yet ſtill he continued for ſeveral weeks to be harraſſed with his cough, to be fluſhed and feveriſh, eſpecially towards the evening, and to complain of burning at the palms of his hands and ſoles of his feet. He never had hæmophthiſis, or expectoration of any kind, till about this time, when he began to cough up viſcid phlegm with much difficulty, on firſt awaking in the morning, and at this period he alſo found his legs and thighs moiſt with cold clammy ſweat towards the concluſion of the night, and was getting much debilitated.

Alarmed at his ſituation, he ſent for me again. I found his averſion to medicine as ſtrong as ever. On entering his room, he cried out, "Do what you "will with me, except doſe me from the apothe-"cary's ſhop." It occurred to me to put him upon Dr. Beddoes's plan of breathing hydrocarbonaceous air. Having explained the manner in which it is ſuppoſed to produce beneficial effects, he ſeemed much delighted, and ſaid, "This promiſes ſome-"thing—this appears to me to be putting the axe "to the root of the tree." He began by inhaling about one part of the factitious air diluted with from eighteen to twenty of atmoſpheric twice a day, in the morning before he got up, and at night after he went into bed, and uſually breathed it for eight or ten minutes. The factitious air was gradually increaſed till it amounted to nearly a ſixth part of the mixture, at which ſtrength it produced a ſlight degree of vertigo or giddineſs, which was uniformly ſucceeded by ſleep; from which he conſtantly awoke refreſhed, and without any of the diſagreeable ſenſations which ſucceed ſleep procured by opium.

After perſiſting in this mode of proceeding, and taking a ſlight decoction of the *cortex ſalicis amygdalinæ* in the courſe of the day, he found his feveriſhneſs, cough, and dyſpnœa much amended, his ſweats gone, his ſtrength improving daily, and at the end of three weeks he rode on horſeback ſix or eight miles every day; and after three weeks more he left Clifton, to return to his own country, almoſt cured of his cough and other ſymptoms; and ſo pleaſed with the new mode of treating diſeaſes of the lungs, that he ſaid he would write to Birmingham for an apparatus to prepare the air for himſelf, or any of his friends who might be affected as he had been.

If it has appeared extraordinary to ſome perſons that air, worſe than that which we ordinarily breathe, ſhould be found ſalutary in pulmonic affections, how will it ſurpriſe people in general, to hear that (in my opinion)

opinion) the air of Clifton-hill and Durdham-down has acquired its well-deſerved celebrity in ſuch diſeaſes, not becauſe it is (as has been pretty univerſally believed) the pureſt air in England; but in conſequence of its being conſiderably *leſs pure* than the air of many other elevated ſituations in the neighbourhood of Briſtol? Obſervation alone is ſufficient to convince any unprejudiced medical man, that this *muſt* be the caſe. The village is ſituated on an eminence, in the vicinity of a great commercial city, in which, and its environs, there are many glaſshouſes, founderies, potteries, brick-kilns, lime-kilns, chemical laboratories, culinary fires, dung-hills, careening-docks for ſhipping, (ſome of them half full of ſtagnant bilge-water) &c. &c. everlaſtingly ſpoiling the air. On the other ſide runs a brackiſh river, which leaves its oozy banks covered with ſtinking mud, putrid animal and vegetable matter, and every kind of filth and offal which it brings down its ſtream from Bath and Briſtol.

But I do not make the aſſertion merely from opinion or obſervation; I proved the fact by experiments with the eudiometer. I brought air in phials from Dundry-hill, and from the eminence that overhangs the village of Wraxall, and having ſubmitted both theſe airs and that of Clifton on the ſame day, when the wind blew from Briſtol, to the nitrous teſt, I found that the air from each of the abovementioned ſituations was ſeveral degrees purer than that of Clifton, or any part of Durdham-down.

As a proof that living in air a good deal injured by the burning of lime, bricks, &c. has been of infinite ſervice in threatened conſumption, I ſhall beg leave to give a ſhort ſketch of two caſes, in which it was ſucceeded by the moſt decided and unequivocal advantage.

CASE I.

The daughter of a reſpectable Quaker laboured under ſeveral well-marked ſymptoms of incipient phthiſis, for ſeveral months, and was at laſt adviſed to go into the country for the benefit of *pure* air. Her father took lodgings for her in the village of Clondalkin, four miles weſt of Dublin, in which there is a very extenſive manufacture of gunpowder, and around which they burn vaſt quantities of lime for manure and building, and make a prodigious number of bricks. The wind happened to blow from a long chain of lime and brick-kilns towards the young lady's lodgings, for ſeveral weeks together; her apartments were conſtantly filled with vapour of a ſtrong, ſuffocating, ſulphureous ſmell.* Her friends, fearing that this might be prejudicial to her, wiſhed her to remove to ſome other ſituation; but, finding herſelf getting better every day, ſhe peremptorily refuſed to change her lodgings. She remained in that foul atmoſphere for four months, at the end of which ſhe returned to town perfectly cured of all her alarming ſymptoms.

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* It was ſo impregnated with fluor-acid air, that it corroded the outſides of the glaſs of the windows.

Case II.

A ſervant aged about ſeventeen, deſcended from phthiſical parents, was affected with cough, fever, fluſhing, debility, emaciation, and other unequivocal ſigns of the approach of the family complaint. I ſent him to the cabbin of a relation in the neighbourhood of the above-mentioned village. The houſe was ſurrounded by a plantation of potatoes of many acres, the leaves of which are known to tranſpire (by day as well as by night) air much worſe than common air.* He gradually grew better, and after a few weeks thought himſelf ſufficiently recovered to return to his maſter's ſervice, in a fine dry open ſituation in the county of Wicklow. He ſoon relapſed; but upon returning to his former abode, and remaining there the whole of the ſummer and autumn, he got entirely quit of all his complaints.

I ſhall conclude this ſection with a few general reflections.

The uſe of factitious airs, as a remedy for diſeaſes which have long remained the *opprobria medicorum*, has been confined within ſo narrow a circle, that they are but rarely reſorted to by the generality of practitioners; few of thoſe even who moſt frequently recommend them to their patients, have experience enough to enable them to point out any determinate modes of exhibiting them, in the great variety of caſes in which they may be required. A great hoſpital, under the direction of able and unprejudiced phyſicians, is the only place in which their efficacy can be aſcertained, or in which the ſtandard of air beſt adapted to particular ſymptoms can be diſcovered. Dr. Beddoes has, I am informed, digeſted a plan for the eſtabliſhment of ſuch an inſtitution; under whoſe management, ſhould the undertaking meet with the encouragement from the publick the importance of it ſeems ſo obviouſly to merit, I dare venture to pronounce that we ſhall, in a few years, be in poſſeſſion of facts that will be of the utmoſt advantage to the community.

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* A candle would not burn in air extricated from potatoe leaves.
INGENHOUSZ's Exp. p. 205.

A BRIEF ACCOUNT OF THE DISCOVERY, COMPOSITION, PROPERTIES, AND USES IN MEDICINE, SURGERY, AND THE ARTS, OF THE OXYGENATED MURIATIC ACID, OR BLEACHING LIQUOR.

For the diſcovery of many of the properties of this moſt uſeful acid, we are indebted to the unremitting attention which the late ingenious and indefatigable Swediſh chemiſt, Scheele, paid to the inveſtigation of every ſubſtance which chance preſented to his inquiſitive mind; but the extenſive application of it to the art of bleaching linen and cotton manufactures, was reſerved for Meſſrs. Berthollet, Bonjour, Conſtant, and Count Bellaing, in France; Mr. Watt, in England; and Mr. Roe (under the auſpices of the Linen-Board) in Ireland.

Scheele, having conceived an idea that it was no more than common muriatic acid deprived of phlogiſton, diſtinguiſhed his newly-diſcovered fluid from that from which he had obtained it, by ſimply prefixing the participle *dephlogiſticated* to the original acid; but the French chemiſts, having diſcovered that it does not differ from common marine acid in being freed from phlogiſtic matter, but in having a large proportion of oxygenous gas added to it, have denominated it OXYGENOUS MURIATIC ACID.*

To generate this acid in a ſtate of vapour, Scheele directs us to take three parts of concentrated marine acid, (which is in ſpecific gravity a fourth part heavier than water) and pour it into a glaſs retort, into which one part of powdered manganeſe had been put; apply a receiver capable of containing twelve ounces. About two drachms or half an ounce of water is to be poured into the receiver before it is affixed to the retort. Place the retort in a ſand-bath, without any lute or other joining of the receiver to it than ſome moiſt brown paper wrapped round the juncture. In about a quarter of an hour the receiver will appear full of yellow vapour. This receiver is to be removed, and another applied, containing a little water as before; and in this manner the receivers are to be changed, as long as the vapour continues to be generated. This vapour is the acid in its aëriform ſtate. The water is of no other uſe than to attract and condenſe whatever portions of the common acid may be volatilized and thrown over unoxygenated.

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* Accounts of experiments made with this acid appeared in the Annales de Chymie, vol. ii. p. 151; in the Memoirs of the Academy of Paris; in the Dictionaire Methodique; and in Rozier's Journal for September 1788.

The properties of this acid are,

1*ſt*. It is of a pale yellow colour, has the ſmell of aqua regia, and affects the lungs with a very diſagreeable ſuffocating ſenſation.

2*d*. It does not (according to Scheele) readily unite with water; and when it has been made to combine with it, it ſcarcely imparts any acid taſte to it, but renders it rather auſtere, and the ſpecific gravity of the ſaturated water is to diſtilled water as 1003 to 1000, when the thermometer is only three degrees above the freezing point. The ſaturated water poſſeſſes all the properties of the aëriform acid, except permanent elaſticity, and is the bleaching liquor of the French chemiſts.

M. Berthollet differs in opinion with Mr. Scheele, with reſpect to the difficulty of ſaturating water with this vapour, and ſays that it unites with water more readily than fixed air does; Mr. Scheele, he imagines, was led into the aſſertion he has made, by not attending to a circumſtance which is very neceſſary to be obſerved, when we wiſh to impregnate water with this vapour, (viz.) the agitation of the fluids frequently during the time the proceſs is carrying on.

3d. It extinguiſhes fire, and kills animals.

4th. This acid does not expel fixed air from aërated fixed alkalies or earths, until it is heated, and then dephlogiſticated air is detached from it. See Kirwan on Phlogiſton, 2d ed. p. 128.

M. Berthollet boiled in a retort, to which a pneumatic apparatus had been fitted, ſome dephlogiſticated marine acid liquor and mineral alkali, and obtained (in addition to the air that was contained in the veſſel) fixed air, and a good deal of air purer than atmoſpheric.

This experiment corroborates the aſſertion of Mr. Kirwan, and is a proof that dephlogiſticated marine acid is incapable of entering into combination with alkalies or calcareous earths, as long as it retains its *full* proportion of dephlogiſticated air.

5th. When this acid unites with aërated alkalies or earths, it forms a catalogue of ſalts entirely unknown to the ancients, viz. oxygenated muriate of lime, of barytes, of pot-aſh, of ſoda, &c. &c. See Lavoiſier's Elements of Chemiſtry tranſlated by Kerr, p. 232.

6*th*. It changes the green colour of martial vitriol to red, but does not affect the colour of vitriols of copper or zinc. It decompoſes and whitens cinnabar, and when the cinnabar, thus altered, is lixiviated with water, a ſolution of corroſive ſublimate is produced.

7*th*. It attacks and diſſolves all metals, even thoſe on which *common* marine acid has no effect, and the ſaline compounds are the ſame as thoſe produced by direct ſolution in marine acid, or in marine acid which had undergone ſome previous preparation or addition. The ſolution of gold yields, when precipitated by volatile alkali, fulminating gold. This proves (indeed its ſuffocating ſmell declares) that it is analogous to aqua regia in its compoſition as well as properties. It alſo ſerves to evince that oxygen is the only ingredient in nitrous acid which enables aqua regia to act upon gold.

8*th*. It does not liquify ice, or diſſolve camphor, in both which it differs from muriatic acid gas.

9*th*. Inflammable ſubſtances reduce this acid to the ſtate of common marine acid, and the contact of light gradually decompoſes it, and produces the ſame effect on it that ſubſtances containing what has been called phlogiſton do.

10*th*. It deſtroys all vegetable colours, and yet it does not, like other acids, *redden* litmus or ſyrup of violets, but renders them *white*; and the colours thus deſtroyed cannot again be recovered by alkalies, or other means.

11th. The leaves of evergreens long reſiſt the action of this acid, and at laſt are only brought to a pale yellow colour.

12th. It bleaches yellow wax, but has an oppoſite effect upon another animal production; for it renders ſilk that has been whitened by art yellow.—Its effects in bleaching wax ſhew that the colouring matter of that ſubſtance is wholly derived from the farina of the flowers from which the bees collect it; for it produces no change whatever upon the colour of animal matters; and hence it is that

13th. It diſcharges the colour of writing-ink, which is compoſed of a decoction of an aſtringent vegetable and vitriolated iron; but it rather ſtrengthens printer's ink, which is made of oil and lampblack. In conſequence of this laſt property it is of great uſe in cleaning old prints, and in rendering old printed papers legible; for it almoſt inſtantaneouſly whitens the paper, and blackens the ink.

M. Berthollet having obſerved the action of oxygenated muriatic acid upon colouring matters in general, it occurred to him that it might produce ſimilar effects upon the colouring vegetable particles that are diſcharged from cloths and threads in the proceſs of bleaching; he therefore had recourſe to experiment, (that only touchſtone by which we can extort truth from nature) and ſoon ſatisfied himſelf that this liquor, under proper management, might be applied with conſiderable advantage to accelerate the proceſs of whitening thread, linen, and cotton.

In his firſt experiments he candidly acknowledges that he employed the oxygenated liquor in a too highly concentrated ſtate, which conſiderably weakened and injured the texture of the cloth he ſubmitted to the experiment. In conſequence of theſe obſervations, he diluted the liquor, and ſucceeded ſo far as to bleach the ſubſtances completely, without injuring or altering their texture in the leaſt degree; although, by keeping, or by ſubmitting them to the action of alkaline lie, they became yellow very quickly. Having reflected upon theſe circumſtances, and compared them with the ſteps in the ordinary bleaching proceſs, he determined to imitate that, and to aſcertain to what it was owing that expoſing linens, &c. to the action of light, air, and water, had the effect of whitening them. With a view to the inveſtigation of this intereſting ſubject, he analyzed the dews, both ſuch as are precipitated from the atmoſphere, and thoſe which are produced by the nightly tranſpiration of vegetables, and diſcovered that both are ſo ſtrongly impregnated with vital air, as to diſcharge the colour of paper faintly tinged with tournſole.

This having ſatisfied M. Berthollet, he reſolved to imitate the ordinary proceſs of bleaching, and by ſucceſſive immerſions of linen intended to be bleached in of alkaline lie and oxygenated muriatic-liquor, he produced a perfect and permanent whiteneſs, The bleaching liquor may be prepared by two proceſſes; with marine acid and mangameſe, or with common ſalt, vitriolic acid, and manganeſe.

The proportions of the ingredients in the former proceſs are three parts by weight of concentrated marine acid, and one of black manganeſe in fine powder; but as the acid can be obtained upon much cheaper terms by the latter proceſs, it is the only one at preſent in uſe. The proportions of the materials in *this* proceſs are—Six ounces of black ore of manganeſe, reduced to fine powder; ſixteen ounces of ſea-ſalt, likewiſe in powder; twelve ounces of concentrated vitriolic acid, and eight ounces of water; and if the manganeſe be known to contain any earth or foreign matter, it muſt be increaſed according to its impurity.

The apparatus for preparing the bleaching liquor, which was invented by M. Berthollet, conſiſts of three parts; diſtilling veſſels of glaſs, a large wooden caſk to contain the water, and a glaſs tube to convey the acid vapour from the diſtilling veſſels into the water in the caſk.

When the materials are ready, the manganeſe and ſalt, both in fine powder, are to be accurately mixed together, and put into the diſtilling veſſel, which is then to be placed in a ſand-bath; the ſulphuric acid, previouſly diluted with the water, and allowed to cool, is then to be poured on the other ingredients, and the tube intended to convey the gas from the matraſs to the intermediate jar, muſt be quickly adapted to the apertures in both veſſels, and the junctures are inſtantly to be well ſecured with proper luting, to prevent the eſcape and diſſipation of any of the gas.

The proportion between the pneumatic caſk and the matraſs ought to be ſuch as to admit of twenty-five gallons of water being contained in the caſk, beſides leaving an empty ſpace, equal to two gallons and a half, which will allow the water to riſe up in the caſk, when it is diſplaced by means of the gas from under the inverted tubes. The caſk being filled with water in this manner, before the proceſs commences, and the ingredients being mixed together in the matraſs, as the gas becomes diſengaged, it ruſhes through the tube of communication into the water. The proceſs is now to be continued without fire, as long as the gas paſſes over with any tolerable rapidity. When the bubbles begin to come over ſlowly, a gentle fire is to be lighted in the furnace, which muſt be gradually increaſed by little and little, till the matters begin to boil towards the end of the proceſs. The approaching completion of the proceſs is indicated by the tube of communication and the intermediate jar becoming hot. During the operation the intermediate jar gradually fills with pure though dilute common muriatic acid. It is neceſſary during the operation to agitate the fluid contained in the tub, by turning the handle of the agitator frequently.

The liquor prepared according to the foregoing proceſs, is of a proper ſtrength for bleaching, without any dilution; but as manganeſe is often found to contain impurities of various kinds, and as it can ſcarcely be expected that the proceſs ſhould ſucceed twice exactly in the ſame way, a method of aſcertaining the ſtrength of the liquor was a great deſideratum in the new method of bleaching. M. Decroiſille entered upon a long ſeries of experiments with the oxygenated liquor, and a great variety of vegetable colours; and at length hit upon a teſt, compoſed of indigo and vitriolic acid, which is found to anſwer the purpoſe of the bleacher perfectly.

The compoſition of Decroiſille's teſt is as follows: —Take one part by weight of indigo reduced to a fine powder, and eight parts of well-concentrated vitriolic acid; digeſt the mixture in a matraſs expoſed to a ſand-heat, until the indigo is completely diſſolved. This ſolution is then to be diluted with one thouſand parts of water, and is to be employed for meaſuring the ſtrength of the oxygenated liquor, in the following manner:—

One meaſure of this teſt ſolution is put into a graduated tube, and the oxygenated liquor is gradually added to it, till the blue colour of the indigo is completely deſtroyed. We then determine, by means of the degrees upon the tube, how many meaſures of the liquor are neceſſary to diſcharge one of the coloured fluid, and by this means, it is plain, we can aſcertain the preciſe degree of ſtrength of the bleaching liquor.

The method of uſing the oxygenated liquor to whiten linen or cotton goods, is as follows. Before the commencement of the bleaching proceſs, the cloth muſt be prepared by ſteeping it for twenty-four hours in water, to extract the dreſſing it received in the loom, and after the water has been well wrung out, it is to be immerſed in the oxygenated liquor for three hours. It is then to be waſhed in alkaline lie, and that being alſo waſhed out with water, it muſt be again ſteeped in the bleaching liquor for about half an hour; it is then to be taken out, and wrung as before, and again waſhed in lie.

The ſame liquor may ſerve for ſeveral repeated immerſions, but care muſt be taken to keep it at its original ſtrength, by adding a ſufficient proportion of newly-prepared liquor, if the firſt appears upon examination with the teſt above-mentioned, to have loſt any part of its gas. It is difficult to determine, *à priori*, how many different immerſions may be neceſſary, but in general from four to eight are ſufficient for cloths made of flax or hemp. When the cloth appears to have acquired a proper degree of whiteneſs, it muſt be rubbed over with ſoap, and then waſhed once more in lie, and receive one other immerſion in the oxygenated liquor.

It is neceſſary to be obſerved, that the lie ſhould be made of cauſtic alkali, which may be done by having one third of its weight of quick lime mixed with the alkali, before the water of ſolution is poured on. After the laſt immerſion, the cloth muſt be plunged into ſour milk, or into water acidulated with a proper quantity of vitriolic acid; about one part by weight to fifty of water, M. Berthollet thinks ſufficiently dilute, but ſome bleachers in Ireland have aſſured me that that is a good deal too ſtrong. The cloth is to be kept in this ſour liquor, about milk warm, for near half an hour; it muſt then be ſtrongly wrung out of the dilute acid, and inſtantly plunged into water; for if the water were allowed to evaporate, the acid would become ſo concentrated as to injure the texture of the linen.

Beſides the above method of bleaching, there has been another introduced into France, in which they uſe an alkali in preparing the bleaching liquor in the firſt inſtance. This has been called after the town in which it was firſt practiſed, the *Javelle Lixivium*, which we are directed to prepare according to the following proportions:—

Take two ounces of common ſalt, two ounces of vitriolic acid, and ſix drachms of manganeſe; and into the jar intended for condenſing the gas, put five ounces of pot-aſh diſſolved in ſixteen ounces of water. This liquor may be diluted with from ten to twelve times its quantity of water, and it has been found to bleach ſubſtances more expeditiouſly than the ſimple oxygenated liquor; but it is ſuppoſed to be better calculated for the bleaching of cotton than of linen fabricks.

The diſcovery ſtood as above related, when M. Berthollet made ſome experiments in the preſence of Mr. Watt of Birmingham, in whom the moſt perfect acquaintance with mechanic powers and moſt profound philoſophic acquirements are united to extraordinary natural abilities. This gentleman, ſoon after his return to England, prevailed upon a Mr. M'Gregor, a bleacher near Glaſgow, to make trials of the proceſs in the great, and M'Gregor was the firſt who uſed the oxygenated muriatic acid in bleaching linen and cotton goods for the market in Great-Britain.

Soon after this, the Commiſſioners of the Linen-Board, wiſhing to have an improvement of ſuch importance to the ſtaple trade of Ireland adopted as early as poſſible in that kingdom, offered a premium of four hundred pounds to the perſon who ſhould produce the beſt ſample of oxygenated muriatic liquor on the firſt of May 1791, and diſcover the proceſs by which it was obtained, and the price at which he could afford to ſell it. This premium was awarded to the late Mr. Robert Roe, chemiſt and ſalt-manufacturer at Ring's-End, near Dublin, who on the enſuing firſt of Auguſt laid the following very intereſting Courſe of Experiments before the Board for the benefit of the public.

EXPERIMENTS ON THE OXYGENATED MURIATIC ACID USED IN BLEACHING, WITH REMARKS ON THE BLUE TEST LIQUOR, &c.

BY ROBERT ROE, M. R. I. A.

WHEN I firſt paid attention to the new method of bleaching with the oxygenated muriatic acid, I found the difficulty that embarraſſed me moſt, was the want of a proper teſt to determine the ſtrength of the bleaching liquid, and thereby to mark the progreſs I made in the different methods I purſued for making it. I ſaw that the practical bleacher was ſtill more in the dark, as he had more purpoſes to anſwer thereby; for it was neceſſary he ſhould know at what ſtrength he bought it, at what ſtrength it was neceſſary to uſe it for different purpoſes, how much it was reduced in the proceſs of bleaching, in order to recruit it, and finally, when it might be thrown away without loſs.

I was aware that M. Berthollet, to whom the world is ſo much indebted for the pains he has beſtowed on this ſubject, had communicated a teſt compoſed of one ounce of fine indigo, diſſolved in eight ounces of vitriolic acid, and diluted with one thouſand parts of water, which, he ſays, was the invention of M. Decroiſille; but the mode which he has ſuggeſted for uſing it in glaſs tubes, has hitherto rendered it uſeleſs, for the fluids are ſo ſtrongly attracted by the glaſs, and being nearly of the ſame ſpecific gravity, they cannot blend, however much they are agitated, or however often they are inverted. Abſtracted from this impediment, I conceive this method could never be uſed to try a ſtrongly impregnated liquor; for inſtance, let us ſuppoſe a liquor 700 ſtrong (which we ſhall meet in the following pages) and that we introduce half an inch of this ſtrong liquor into a glaſs tube, then we muſt neceſſarily have 700 half inches of tube (nearly 30 feet) in order to determine its ſtrength, which we ſee is totally impracticable.

I am ſatisfied M. Berthollet never tried this experiment on liquors of great ſtrength; for if he had, the defects I have mentioned could not have eſcaped ſo acute an obſerver; yet no part of the whole proceſs requires preciſion more than this; for whether the prepared liquor be weak or ſtrong, by knowing how to dilute it in proportion to its ſtrength, its operation in bleaching will be uniform; and without this knowledge any faults committed through the ignorance of the operator, will be charged to the inſufficiency or uncertainty of the bleaching liquid, and eventually bring its uſe to be conſidered as injudicious and dangerous to the trade.

But notwithſtanding the impracticability of uſing the teſt I have mentioned in glaſs tubes, yet in another and more ſimple method of treatment it will be found to be the beſt teſt that has yet been diſcovered.

It is the moſt uniform and the cheapeſt that ever was invented. The mode I allude to is, to have ſmall meaſures properly proportioned to each other, and when the liquid is ſtrong, to prevent waſte of blue, and a tedious repetition of meaſures, let a ſmall meaſure of the liquor to be tried be put into a meaſure containing twenty-four of the ſame meaſures of water; it then becomes diluted to a twenty-fifth part. To a meaſure of this diluted liquor add as many meaſures of the blue teſt as it will diſcharge, which multiplied by twenty-five gives its whole ſtrength. It will be proper to have a meaſure of five, for the ſake of diſpatch in adding the blue teſt liquor. This will give ſome idea of the experiment; but to perform it accurately, will require ſome practice, or ſeeing it performed. It is neceſſary that the experimenter ſhould ſit low enough to view his meaſures horizontally, in order that they may not be over-filled, otherwiſe he may be deceived.

The apparatus which I made uſe of firſt was that deſcribed by M. Berthollet. I found, however, that when the liquor grew very ſtrong, the internal work was deſtroyed, and became almoſt tranſparent and as ſoft as a turnip. I laid this by, and ſubſtituted another in which no wood was employed.

The ſecond apparatus was compoſed of vitriol bottles fixed on a loft, with their necks turned down through the loft, from whence glaſs tubes deſcended till they received the gas through a curved tube at the neck of the retort, immerſed, however, at the juncture in a crock of the fluid to be impregnated on the principle of the pneumato-chemical experiments; the vitriol bottles were perforated in the bottoms, in order to receive the charge. Glaſs cocks were fixed in the deſcending tubes, to prevent the falling of the liquor while the hole above was open, which when all was cloſed was kept up by the preſſure of the atmoſphere until the elaſticity of the aſcending gas forced it down into the receiver, from whence it was put up as often as was found neceſſary until fully impregnated. This apparatus anſwers exceedingly well, but being too intricate for the management of many who may have occaſion to purſue this proceſs, I laid it aſide upon the preſent occaſion, and ſubſtituted the ſimpleſt that I could poſſibly deviſe. This conſiſts merely of tubulated retorts with vitriol bottles for receivers; many of which can be attended by a ſingle man, and with the ſmall addreſs of agitating the receiver when filled with gas, will ſucceed as well as the moſt complicated apparatus that has yet been contrived.

In uſing retorts it ſhould be obſerved that M. Berthollet's proceſs with muriatic acid and manganeſe has been purſued; but in the proceſs (called the Javelle method) where common ſalt, manganeſe, and vitriolic acid are uſed, I have found a kind of alembicks, with ſtone-ware bodies and glaſs heads fitted thereto, the propereſt veſſels for the purpoſe, becauſe the cake of Glauber ſalt and manganeſe, remaining after diſtillation, can rarely be got out of a retort without breaking it.

Having ſaid ſo much on the ſubject neceſſarily preparative to underſtanding the experiments, I ſhall add a few, that appear moſt intereſting in practice.

No. I.

Strong by the Blue Teſt.

1 lb. manganeſe pounded and ſifted
2 lb. muriatic acid in the retort
1 gallon of mild lye in the receiver, 9 lb. per gallon.

Evening		Next morn.
333	....	550

The ſpecific gravity of the muriatic acid 1,163.

No. II.

1 lb. manganeſe
2 lb. mnriatic acid
2 quarts of the ſame lye as above
2 quarts of water, weight per gallon 8 lb. $7\frac{1}{2}$ oz. when mixed.

Evening		Next morn.
333	....	546 or better

Theſe two experiments were made to determine this important fact, viz. Whether alkali (the moſt expenſive article in the proceſs) might not be diminiſhed without injury. The reſult proves that it may; for we ſee the lye, when reduced to half its ſtrength, under equal circumſtances, was equal (as nearly as could be determined) to that of the whole ſtrength. This lye contained one pound of pearl-aſh to one gallon of water. The Javelle liquor, mentioned by M. Berthollet, ſtates five ounces of alkali to ſixteen of water, that is, 2½ to 8.

No. III.

Cauſtic lye made by adding 38lb. of quick lime to 114lb. of pearl-aſh, and made to weigh about 9lb. per gallon, as in the firſt experiment.

	Evening	Next morn.
1lb. manganeſe 2lb. muriatic acid in the retort 1 gallon of the above lye in the receiver.	700	850

Here we ſee another important fact, viz. that cauſtic lye is more ſuſceptible of imbibing the gas, and retaining it than mild lye of equal ſtrength; and we ſee, that under equal circumſtances, (as might be expected) it becomes conſiderably ſtronger.

It may not be improper here to mention, that when mild alkaline lye becomes highly charged with gas, a cryſtallization takes place of ſmall lamellar ſalt like the ſcales of fiſh, which poſſeſſes the extraordinary property of deflagrating in the manner of nitre, and alſo that of fulminating when joined to inflammable ſubſtances.

It ſhould be obſerved alſo, that when this cryſtallization takes place, the bleaching liquid becomes good for nothing; and at the ſame time an air is endeavouring to make its eſcape with ſuch force as to drive the cork out of the bottle. I ſuppoſe it vital air, but have not yet examined it.

M. Berthollet mentions a cryſtallization taking place, but is not particular as to any of its qualities.

No. IV.

To determine whether the mangane ſe in the retort was quite exhauſted of its gas, the following experiment was undertaken:

The reſidua of ſeveral retorts were taken, waſhed, put into a freſh retort, and muriatic added in the uſual proportion.

	Evening
1lb. already uſed manganeſe 2lb. muriatic acid 1 gallon lye	33

½lb of freſh manganeſe was added, and the ſtrength next morning was 380

No. V.

An Experiment with Water.

	Evening	Next morn.
1lb. manganeſe 2lb. muriatic acid 1 gallon of water	80	80

We ſee in this laſt experiment that water, under equal circumſtances, aroſe to but 80. The operation was inſupportably offenſive, though conducted with the ſame care as the others, and the liquor alſo very offenſive, and ſo incapable of retaining the gas, that the firſt and ſubſequent experiments made to determine its ſtrength diſagreed.

I will not ſay that the liquid made on water cannot be uſed to advantage in bleaching by this method; but I can ſafely ſay, it muſt always be made on the ſpot where it is to be uſed, as it will not retain the gas for any conſiderable time.

Thoſe who uſe lie for this purpoſe ſhould impregnate it highly with the gas, as the lie is generally loſt after being diluted, and its oxygenated gas exhauſted.

It ſhould be obſerved, that a very fetid ſmell is a mark of its impurity, and ſhews that muriatic acid has been driven over unaltered. This may happen by too much fire being given,* and may be prevented by an intermediate veſſel being uſed, and in a great meaſure by keeping the noſe of the retort elevated above the level, in order that any acid that condenſes may fall back again. When lie is uſed for this purpoſe, acid in a ſmall quantity is of very little moment, for it is inſtantly converted into a neutral ſalt, uſually called Salt of Silvius. I have taken a ſolution of Salt of Silvius inſtead of lie, and impregnated it with gas, and found it would bleach; but it was a bad article, and by ſuch neutralization a portion of the alkali may be ſaid to be nearly loſt.

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* The heat of the liquor in the retort ſhould be about 140.

Theſe experiments are ſelected from many others that were made at the ſame time, as promiſing to be of ſome uſe in practice; they were not intended for ſuch a haſty publication, but being conſidered worthy of notice by ſome gentlemen who were judges of the ſubject, I could not refuſe communicating them. Theſe have, however, ſuggeſted other experiments, which at a future day I hope may prove of ſervice to the public.

Before I cloſe the ſubject, I ſhall communicate an idea that offers itſelf upon the preſent occaſion, which it is probable may be worthy of the notice of the extenſive bleacher; that is, by having a ſand-bath at a convenient diſtance from the veſſel in which he intends to ſteep his linens, and having charged it with ſuch lie as may ſuit his particular purpoſe, he may, by the help of a few retorts and glaſs-tubes, in the courſe of the night, impregnate the whole liquor, and determine the ſtrength of the impregnation by the blue teſt. Should ſuch a mode be ever put in practice, it will be neceſſary to have the lie ſtirred round from time to time, in order to facilitate its abſorbing the gas.

* * *

The celebrated chemiſt Weigleb *aſſerts*, that expoſure to the rays of light decompoſes the oxygenated muriatic acid, and reduces it to its original ſtate of *common* marine acid; (ſee Weigleb's Chemiſtry, p. 32) but M. Berthollet *proves* the fact by a direct experiment. He took ſome water impregnated with this acid, and put it into a bottle, from the mouth of which a tube, properly bent, was introduced below the receiver of a pneumato-chemical apparatus filled with and inverted in water, and expoſed the bottle to the rays of light; the reſult of which was, that air-bubbles of pure oxygen air were extricated, and the liquor became ſimple dilute muriatic acid, or ſpirit of ſea-ſalt; (ſee Eſſay on Bleaching, p. 16 and 17) and light has the power, which is not poſſeſſed by heat alone, of diſengaging and reſtoring to its aëriform ſtate the oxygen, which was combined with muriatic acid, in ſome degree deprived of its elaſticity. In this proceſs it appears that light and heat enter into combination with oxygen, and that to this union the elaſtic form of vital air is owing, which, in loſing its aëriform ſtate during rapid combuſtion, again allows the principle of light to eſcape, whilſt at the ſame time a large quantity of *ſenſible* heat is produced. (See Eſſay on Bleaching, p. 19.)

It is ſufficient for the manufacturer merely to be apprized of the effects which light has upon the bleaching liquor, to enable him to guard againſt it, by keeping it and the troughs in which he performs the proceſs of bleaching as much in the dark as poſſible; but the man of ſcience will not be content,

unleſs

unleſs he can account philoſophically for the phenomenon.

I will endeavour to ſhew that this can be done according to both the old and new chemical theories.

It was a fundamental tenet of the former doctrine, that phlogiſton was connected with, or made a part of the rays of light, and that it was brought down to the earth with them from the great ſource of heat and light, the ſun. This being premiſed, and it having been mentioned above, that the difference between the common and the oxygenated acid conſiſted, according to Scheele, in nothing more than ſimple privation of phlogiſton; it is obvious that as ſoon as the dephlogiſticated acid ſhall have attracted a ſufficient quantity of the principle that had been diſengaged from it, from the rays of light, it will reaſſume its original form, and be reconverted into *common* marine acid.

Such is the explication which the Stahlian theory enables us to give. The Lavoiſierian preſents us with a no leſs ſatisfactory and concluſive ſolution of the phenomenon.

The adherents to this doctrine have proved, that the difference between the acids does not conſiſt in the privation of phlogiſton, but in the addition of oxygen. Light, aſſiſted by the green leaves of vegetables, poſſeſſes the power of detaching oxygen from its combination with hydrogen (though their attraction is very ſtrong) in water, even through glaſs jars; how eaſily then may not light alone effect the ſeparation of it from marine acid, for which its affinity is very weak indeed!

It has been queſtioned whether the adoption of the new mode of bleaching would tend to the advancement of the manufacture of linen. It might as well be doubted whether the invention of ſpinning-jennies, and other machinery, has been of advantage to the woollen and cotton manufactures. Every improvement in an art, which diſpenſes with a number of hands, and accelerates a material proceſs, muſt, moſt incontrovertibly, enable the manufacturer to bring his goods to market more expeditiouſly, and on more reaſonable terms, to his own great emolument, and the good of the public.

The manufacturer of linen, in the populous and not very fertile province of Ulſter, complains that the price of proviſions, and conſequently of labour, has become extravagantly dear in that country, on account of the number of hands he is obliged to employ, and the quantity of land he is under the neceſſity of ſetting apart excluſively for the purpoſe of a bleach-green. But if the new bleaching proceſs were generally known and practiſed, a third of the ground and men now engaged in the buſineſs would be ſufficient;* which, it is very obvious, could not fail to be attended with the moſt beneficial conſequences to the commerce and proſperity of Ireland.

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* The other two-thirds of the ground, when cultivated by the men, would ſoon leſſen the price of potatoes and oatmeal.

DIRECTIONS FOR USING THE NEWLY-INVENTED BLEACHING LIQUID FOR WHITENING LINEN, TAPE, THREAD, AND THREAD STOCKINGS.

Every kind of new goods is firſt to be put to ſoak in water for two days, to purge or free it from ſpittle, the dreſſing it received in the loom, &c. It is then to be waſhed out of the water, and boiled ſlowly for two hours in pearl-aſh lie, after which it is to be rinſed a ſecond time in cold water, and then immerſed in the bleaching-liquid, diluted with twenty-five waters, and kept ſwimming, frequently ſtirring about in the mixture for an hour; after which it is to be covered up cloſe and left at reſt for five hours, when it is to be rinſed through three waters, and dried in the air, boiled again in lie, and immerſed in the liquid as at firſt. This proceſs is to be repeated three times, after which the goods are to be put into a weak ſouring of vitriolic acid, for an hour or two, often ſtirring them, and immediately after they are taken out of this, they are to be waſhed three times through cold water, and finiſhed in the common way. All kinds of cotton goods, ſuch as callicoes, muſlins, ſtockings, &c. are to be treated as linens, only that the liquid may be diluted with thirty inſtead of twenty-five waters. The ſame mixture will anſwer a ſecond time for fine goods, provided it be uſed next day, with the addition of half the quantity of liquid as at firſt.

To take the ſtains of every kind of wine and fruit out of table linen.

If the ſtains are of long ſtanding, the cloths, napkins, or the like, are to be put into as much of the liquor, diluted with twenty-five waters, as they will ſwim in, often ſtirring them for an hour, then waſhed and put on the graſs to dry; but if the ſtains be freſh, you may add thirty waters to the liquid. Callico Printers, to diſcharge colours (except ſuch as are prepared from metals, on which the bleaching liquid will take no effect) from the parts of the linen or cotton fabricks they intend ſhould be white, put the goods into as much of the liquid as they will ſwim in, ſtirring them about for an hour, as above directed for linen, then cover them up till the colour is diſengaged; after which, waſh, dry, and dreſs them, as if they had been bleached in the common way.

How to uſe it in the manufacture of fine paper.

All kinds of linen and cotton rags are to be ground to a *half pulp*, and then put into the liquid diluted as for linen; if one ſteep does not do, give them another.

This liquid is alſo known to take out what bleachers called *ſprits*, which frequently remain in linen, and cannot be removed by the ordinary mode of bleaching; and all ſuch pieces of linen as are difficult to be whitened in the common way, and which are conſequently often obliged to be laid by till another ſeaſon, may be perfectly bleached in a few days in the above way, and rendered fit to be ſent to market.

To make the lie.—In every gallon of water diſſolve ſix ounces of pearl-aſh.

To make the ſouring.—Two ounces of vitriolic acid to every gallon of water.

* * *

There has been but little uſe, as yet, made of the oxygenated muriatic acid, in the practice of medicine or ſurgery. Mr. Cruikſhank has employed the oxygenated muriate of pot-aſh with ſucceſs, as a cure for ſiphilis; and I have ſeen the oxygenated *water*, not the *lie*, of conſiderable advantage in deterging or cleanſing foul phagædenic ulcers. From the yellow or greeniſh appearances, ſo well known to ſurgeons, it in a ſhort time makes them florid, and diſpoſed to incarnate. Query, therefore, would it not be a better ſubſtance than water to wet the bandage with, in the mode of treating ulcers lately recommended by Mr. Baynton?

SUMMARY OF THE MOST REMARKABLE PROPERTIES OF OXYGEN.

It is the principle of acidity. And hence, as I have already ſhewn, the term is derived from the Greek *οξυς, acide,* et *γεινομαι, j'engendre.* (*Encyclop. Meth. Chym.* i. 640.)

With ſulphur it forms ſulphuric or vitriolic acid; with azote, nitrous and nitric acid; with phoſphorus, phoſphoric acid; with muriatic radical, muriatic acid; with carbon, carbonic acid, or fixed air, &c. &c.——*Lavoiſier's Chemiſtry*, edit. 3d, p. 255.

When combined with caloric, or the matter of heat, it forms *pure, vital,* or *highly reſpirable air,* now called *oxygen gas.* The weight of this gas, at the temperature of 54.50, and under a preſſure equal to 28 inches of the barometer, is half a grain for each cubical inch nearly, or one ounce and a half for each cubical foot. It forms almoſt a third part of the maſs of the atmoſphere.

It is the only elaſtic fluid of which the atmoſphere is compoſed, that is reſpirable, or that is capable of ſupporting flame, or of aſſiſting in the calcination of metals.

The oxygen which exiſts in the atmoſphere is the ſource from which the colour and heat of the blood is derived; hence it is, that when the ſyſtem is ſurcharged with it, the cheeks, lips, gums, tongue, &c. are more florid than common, and that the whole body is unuſually heated.

It reſiſts putrefaction; but not ſo powerfully as fixed air.

It enters into the compoſition of animal ſubſtances. Fleſh is compoſed of oxygen, hydrogen, carbon, and azote; fat, of oxygen, hydrogen, and carbon; it promotes the change of fleſh into a fatty matter, which is now found to be very analogous to ſperma-cæti in its properties. *See Dr. Gibbes's experiments on the converſion of animal matters into a ſubſtance reſembling ſperma-cæti, p.* 38.

Aſſiſted by light, I have ſhewn, it gives colour and odour to the leaves and flowers of plants, and flavour to their fruit.

It is alſo the colouring matter of the calces, or oxyds, of all the metals, and in ſhort of all pigments.

'In what manner (argues the ingenious chemiſt 'Chaptel) do the coloured bodies of the three kingdoms of nature acquire the property of conſtantly 'reflecting one determinate kind of rays?

'It appears that the three colours which are the 'moſt eminently uſeful in forming all the others—'that is to ſay, blue, yellow, and red—are developed 'by a greater or leſs abſorption of oxygen; and hence 'we ſee that iron, when juſt beginning to undergo 'calcination, either by heat or the action of a weak 'acid, aſſumes a deep blue or ſanguine colour: and 'metals acquire the property of reflecting yellow, 'when they are combined with a greater quantity 'of oxygen; and accordingly we perceive this colour 'in moſt of them, in proportion as the calcination 'advances. Maſſicot, litharge, ochre, orpiment, and 'mercurius emeticus flavus, are inſtances of this. 'A ſtronger combination of oxygen produces red, 'as we ſee in minium, colcothar, red precipitate, &c.

'The blue colour is formed in *dead* vegetable 'ſubſtances only by fermentation, which fixes the 'oxygen in them; and all vegetables which are na'turally blue, or of a colour in which blue predo'minates, are converted to red by the addition of 'an acid, which we know imparts its acidifying prin'ciple (that is, oxygen) to them.

'When fleſh meat putrefies, the firſt impreſſion of 'the oxygen conſiſts in producing a blue colour; 'and hence the blue appearance of mortifications, of 'fleſh becoming putrid, and of game too long kept. 'This blue is ſometimes ſucceeded by red, as we ſee 'in cheeſes, which firſt become covered with blue 'mouldineſs, and afterwards grow reddiſh.'

Thus then we ſee that it is the principle (as Dr. A. Fothergill ſuppoſed it was) which reddens the the blood, and tinges the cheek of beauty; which diffuſes elegant ſhades of the ſame colour over a conſiderable part of the vegetable creation; which blooms in the roſe, the tulip, and the carnation; which glows in the weſtern ſky, &c.

It is the proximate cauſe of irritability; and hence we ſee that irritability is increaſed by breathing pure vital air; that the heart, when ſtimulated with blood that has juſt received oxygen in its paſſage through the lungs, poſſeſſes more heat and irritability than any other muſcle of the body; and that its right cavity contains a greater quantity of heat evolved in a ſenſible form, and is more irritable than the left, which receives it in a latent ſtate. The vital motion, therefore, of the right ſurvives that of the left.

Hence, alſo, we ſee why the calces of metals are far more active medicines than the metals themſelves; and, finally, hence we ſee why vital air affords the moſt effectual antidote againſt the baleful effects of mephitic vapours, putrid animal effluvia, and other ſpecies of noxious air, which ſuddenly extinguiſh life.

APPENDIX.

TO GENERATE FIXED AIR.

PUT one ounce of white marble duſt into a large pneumatic apparatus with a bent neck, and pour on dilute ſulphuric acid, conſiſting of one ounce of ſtrong ſulphuric acid mixed with ſix ounces of water: this quantity of dilute acid may be poured on the marble duſt at intervals, ſo as to keep a conſtant ſtream of air diſcharging.

OXYGEN GAS.

Put ſix or eight ounces (according to the ſize of the retort) of bruiſed cryſtals of the pureſt nitre into a coated earthen retort, heat the retort gradually till it becomes red-hot throughout, at which period of the operation the air will be very pure.

Or, put two or three ounces of minium into a pneumatic apparatus, and pour on as much ſtrong ſulphuric acid as will but juſt moiſten it, adapt the bent neck, and towards the end of the proceſs, heat the charge (if neceſſary) with the flame of a ſpirit lamp.

HYDROGEN GAS.

Fill a pneumatic apparatus with ſmall iron nails, and pour on them, ſo as nearly to fill the glaſs, dilute ſulphuric acid, conſiſting of one part ſtrong acid, and five of water; when the charges ceaſe to work, pour out the ſaturated ſolution, nearly fill the glaſs with freſh dilute acid, and proceed as before.*

NITROUS AIR.

On copper wire, coiled into ſmall rings and put into a pneumatic apparatus, pour, ſo as nearly to fill the glaſs, dilute nitrous acid, conſiſting of one part nitrous acid, ſp. grav. 1500, diluted with five parts of water.

Sulphuric acid air may be procured by heating with a flame of a ſpirit lamp, a charge, conſiſting of one part mercury and two parts ſulphuric acid; it may be made in a ſmall glaſs retort, or a pneumatic apparatus; the latter is preferable, as the neck of the retort becomes wet with the acid, which afterwards diſtils over on the ſurface of the water or mercury.

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* It may be imagined, from a paſſage in page 23, that I am of opinion that the hydrogenous-gas is extricated from the metal in this proceſs. When the notes, from which that paſſage was tranſcribed, were written, it was univerſally believed that it did proceed from metals; but it has ſince been proved to be produced by the decompoſition of the water, as may be ſeen in the following experiment:

Dr. Fordyce aſcertained how much alcaline ſalt a given quantity of ſulphuric acid was capable of neutralizing; and afterwards collected as much hydrogenous-gas from a certain proportion of granulated zinc, water, and the ſame acid, as the materials could be made to give out, after which he weighed the ſolid reſiduum and found that it had loſt nothing; he then added as much alkali to the fluid as he knew it ought to ſaturate, and ſatisfied himſelf that that ſubſtance had not been deprived of any principle. It is, therefore, evident that the gas is obtained from the water, and not from either of the other matters employed in the proceſs.

The moſt ſimple way of procuring azotic-gas, is by inverting a glaſs jar over a candle in a pneumatic trough, filled either with quick-ſilver or water; as ſoon as the candle burns out, we may be certain that the oxygen of the air is all conſumed, and the azote left behind. This, however, is not very pure azotic air, for it contains a little fixed air, which reſults from the combination of carbon, produced by the combuſtion, with the oxygen that was in the air, originally incloſed in the jar.

HYDROCARBONATE.

When the ſteam of boiling water is made to paſs over the ſurface of red-hot charcoal, it yields abundance of a peculiar ſort of inflammable gas, called *hydrocarbonate*.

In the preparation of hydrocarbonate, Mr. Watt recommends the charcoal of the ſofter woods, ſuch as willow, poplar, hazle, birch, or ſycamore, avoiding ſuch as have reſinous or aſtringent juices. Prepare the charcoal by heating it to full ignition in an open fire, and quenching it in clean water; in which caſe it will be free from any bituminous matter, which might conaminate the air, as generally happens with common charcoal.

* * * * * *

Raiſing its temperature produces no other alteration on common air, than merely an increaſe of bulk. Every degree of Fahrenheit's thermometer rarifies, or increaſes the bulk of common air, about $\frac{1}{472}$d part of the whole.

FINIS